THE MASTERWORK OF JULES TISSOT

ENGLISH EDITION

VOLUME III

This translation of J. Tissot's 1946 work 'Constitution Des Organismes'
is based on the original French edition, which is in the public domain.
Translated by **Someone for Everyone** in 2025
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Dedication

To all who search for meaning
in the smallest of things, and to
the unseen forces that give life
its Beginning and its End

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Jules Tissot

Qouted from the *''Cursed Scientists – Researchers Excluded''* by **Pierre Lance**[1]
(Translated from French)

''Dr. Jules Tissot, professor of general physiology at the National Museum of Natural History, devoted most of his life to in-depth research on the elements composing the living cell. Although he differed from Béchamp on several points, he nonetheless acknowledged the eminent value of his predecessor's work and, like him, spoke out against Pasteur's serious errors.

Although differently oriented, the theses of Béchamp and Tissot complement and mutually confirm each other on the essentials, namely the extreme polymorphism of living matter and the importance of the evolution of internal microelements in the appearance and development of plant, animal, and human pathologies. Both demonstrated with equal rigor and vigor the falsity of Pasteur's dogmas and the excessive importance given by the Pasteur school to external microbes, a concept that would lead all medical prevention into the harmful but lucrative system of excessive vaccination.''

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''One might be surprised that Tissot is even less known than Béchamp, since Tissot worked closer to our time, before, during, and after the Second World War. And some readily believe that the closer we get to the 21st century, the more scientific discoveries freely blossom in the open in the fireworks of "marvelous progress." It is to be feared that this excessive naiveté will prepare these good people for some painful awakenings. For we are in reality surrounded by false progress which, after contaminated blood and mad cow disease, mere warnings, is surreptitiously preparing us for hallucinatory health catastrophes.

The cruel reality is that the only scientific or supposedly scientific discoveries that can develop, particularly in the last century, are those that open up careers for financial powers, which, once established, have the means to nip in the bud anything that threatens their supremacy, and they do not hesitate to use them…''

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''If there was one point on which Tissot erred, it was in assuming that his "bomb" would actually blow anything up. As he quite rightly pointed out, we had been sinking deeper into error for three-quarters of a century. And indeed, we were so deeply entrenched in it that it would have taken a veritable earthquake for the scientific world to regain its senses. These earthquakes began to occur toward the end of the 20th century, with AIDS, the explosion of cancers, the unexpected return of tuberculosis, multiple sclerosis,

Creutzfeldt-Jakob disease, and the list goes on. But all of this came too late to vindicate Tissot, who in the meantime had sunk into the oblivion his enemies had so carefully prepared for him.

In his time, as we have seen, Tissot was silenced by the scientific community, even more radically than Béchamp had been. Perhaps they both should have jumped over their adversaries, so to speak, and tried to address the public directly in order to show them the disastrous consequences that the errors of Pasteurian bacteriophobia, which was in the process of "phagocytizing" the medical world, would bring about in the following decades. They hadn't thought of it, and even if they had, could they have succeeded?

It's highly doubtful, because it's hard to imagine which publisher would have had the audacity to publish and distribute in bookstores texts rejected by official scientific bodies, in these fields where it's difficult for the layperson to judge for themselves and disregard expert opinions. Another biologist, long shunned by the scientific establishment, accomplished this feat: Jean Rostand. But he had a considerable advantage: bearing an already famous name, which opened the doors of major publishing houses to him.
Neither Tissot nor Béchamp had a weapon of this caliber in their arsenal.''

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*1) Pierre Lance ‘’**Savants Maudits – Chercheurs Exclus**”*
Un requisitoire implacable contre la ‘’nomenklatura” scientifique.

DOCTÉUR J. TISSOT
Professeur honoraire de Physiologie Générale
au Muséum national d'Histoire Naturelle

CONSTITUTION DES ORGANISMES

ANIMAUX ET VÉGÉTAUX

CAUSES DES MALADIES

QUI LES ATTEIGNENT

◆

3e VOLUME

CONSTITUTION ANATOMIQUE DE LA MATIÈRE VIVANTE

FONCTION BACTÉRIENNE DES ÊTRES VIVANTS

VIRUS DES MALADIES AUTOGÈNES ET HÉTÉROGÈNES
ET LEURS SOURCES ORIGINELLES

PREUVES DE L'INEFFICACITÉ ET DES DANGERS
DES VACCINATIONS ACTUELLES

PARIS
AU LABORATOIRE ANNEXE DE PHYSIOLOGIE GÉNÉRALE
DU MUSÉUM D'HISTOIRE NATURELLE
42, BOULEVARD SAINT-MARCEL

1946

DOCTOR J. TISSOT
Honorary Professor of General Physiology at the
National Museum of Natural History

CONSTITUTION OF ORGANISMS

ANIMALS AND PLANTS

CAUSES OF DISEASES

WHICH AFFECT THEM

◆

3e VOLUME

ANATOMICAL CONSTITUTION OF LIVING MATTER

BACTERIAL FUNCTION OF LIVING BEINGS

VIRUSES OF AUTOGENOUS AND HETEROGENEOUS
DISEASES AND THEIR ORIGINAL SOURCES

PROOFS OF THE INEFFICACY AND DANGERS OF
CURRENT VACCINATIONS

PARIS
AT THE ANNEX LABORATORY OF GENERAL PHYSIOLOGY OF
THE MUSEUM OF NATURAL HISTORY
42, BOULEVARD SAINT-MARCEL
—
1946

I dedicate this volume to all the

DOCTORS OF FRANCE

Hoping that the new concepts contained in this book, demonstrating the total ineffectiveness of certain vaccinations and their dangers, will encourage them to take the necessary action in their professional councils, unions, and associations to have the odious law of June 25, 1938, which deprived the French of the inalienable right to freely dispose of themselves and to protect the health of their children, repealed.

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Important Note

In several chapters, a reference grid, attached to the plates, has been used to precisely locate the points covered by the demonstrations. To use it, align the two horizontal and vertical lines of the lower-left right corner of the grid exactly with the two lines of the lower-left right corner of the figure indicated. The indication given in the text includes a letter (or several) followed by a number (or several). The object thus indicated is located at the intersection of the horizontal and vertical lines passing through this letter and this number.

INTRODUCTION

In this third volume of the work: *Constitution of Animal and Plant Organisms, Causes of the Diseases That Affect Them*, I have finally been able to gain knowledge of the morphological constitution and organization of living matter, animal and plant, and of the two elementary organelles of a bacterial nature that constitute it; these are:

On the one hand, the dumbbell organelle which, from top to bottom of the scale of living beings, forms the fixed framework that is the very substance of the organized living matter of their organism.

On the other hand, the mobile bacterial organelle, the colibacillary organelle for mammals, which vegetates in the albuminous liquid medium circulating in the meshes of the fixed framework of living matter and which carries out the chemical actions necessary for the conservation and manifestation of the latter's properties.

The first, the dumbbell organelle, performs the constructive function of tissues and organs. The second, which is the micrococcal granulation of the colibacillus, performs the crucial function of carrying out the chemical or fermentative actions of the organism. It participates in part in the constructive function for the construction of only a part of the connective tissue, the elastic fibers: it certainly participates when it comes to the repair of damaged parts of the organism. Thus, in every living being, life consists essentially of the transformation, by the bacte- rial organelle, of the food substances intended to provide the materials and energy necessary for the manifestation of the vital properties of the dumbbell organelle, which is organized living matter itself.

These two functions, one constructive, the other chemical, of the two organelles, very different in form and properties, are unknown to general physiology. Knowledge of the first was the consequence of the elimination of a false biological notion considered of capital importance, that of the existence of mitochondria, due to gross errors of observation.

As for the second, it is the continuation and consecration of the research presented in the first volume, establishing the mycelial and bacterial nature of living organisms. It is above all the result of the continuous study, over more than twenty years, of the mechanism of blood coagulation and the meticulous determinism of the conditions and causes of this phenomenon.

* * *

These two new functions, performed by two elementary organelles of a bacterial nature, are in formal opposition to Pasteur's dogmas, whose inaccuracy they establish.

For three-quarters of a century, four false dogmas, introduced into science by Pasteur and which can be described as catastrophic, have halted progress in bacteriology and the fight against diseases in living beings, both animals and plants, at the same time as they placed an insurmountable obstacle to the progress of general biology and physiology with regard to numerous points of the greatest importance for these two branches of science. These four false dogmas are:

1. The dogma of atmospheric panspermia.
2. The dogma of the asepsis of living organisms.
3. The dogma of bacterial monomorphism.
4. The dogma of contagion.

These dogmas have had a harmful effect because they have completely distorted the reasoning of the artisans of scientific progress by instilling in them inaccurate notions about the nature and properties of living matter and by directing their work in false directions.

These false notions have had the effect of establishing in their minds a fundamental distinction, an irreconcilable difference of nature between the living matter of organized beings and that of bacteria, especially because the latter are considered the virulent agents of diseases.

It results from this state of mind that any scientific result which tends to establish a connection between organized living matter and bacterial living matter is immediately attacked, fought, not with the aim of verifying it, but with the sole aim of preventing its development, its disclosure, that is to say, of stifling it.

And now the new notions contained in this book, which material facts can only confirm, establish that these organized beings are entirely constituted by two organelles of a strictly bacterial nature, and that these, in numerous abnormal circumstances, themselves become virulent for the organism they constituted.

One, the dumbbell organelle, becomes, through a deviation or degeneration from its normal state, the agent of the two greatest scourges of humanity, cancer and tuberculosis and probably also of leprosy.

The other, the colibacillary organelle, can become, through degeneration, the agent of the majority of human diseases, diseases caused by colibacillary, staphylococcal, streptococcal, pneumococcal agents, etc., the latter being confused with the colibacillus.

These new notions definitively destroy pasteurian dogmas and make them unsustainable in the future. They will be violently attacked by a school that, for three-quarters of a century, has defended the false and harmful dogmas listed above and wants to maintain them at all costs despite any appearance of truth.

This will not be new and will only be the continuation of the attacks I was subjected to in 1926 and 1936, on the occasion of the publication of the first two volumes of this work, and the continuation of the attacks against all those who have published notions contrary to these dogmas: **Frémy, Béchamp, Galippe, Portier.**

How could such a situation, so detrimental to the progress of science,
have been created?

*** * ***

The primary origin of this situation lies in **Pasteur's** stubborn denial of the intracellular origin of the ferment in grape juice and his stubbornly maintained affirmation of its atmospheric origin. As a consequence of this false principle, he also supported the atmospheric origin of the ferments that putrefy materials of animal origin, denied their intra-organic origin, and created the false dogma of the asepsis of living organisms by asserting that the bodies of animals are closed, in ordinary cases, to the introduction of germs from lower beings.

The consequence of these false principles was that, applying them in his memoir entitled *Research on Putrefaction* (1853), he drew false and completely unfounded conclusions. **Béchamp**, by means of clear, precise, and easily controlled experiments, demonstrated, beyond the slightest doubt, the falsity of these conclusions, first for meat, then for blood, milk, and urine; he proved that the cause of the putrefaction of these materials was due to the granulations of intra-organic origin that these materials contain, and which he called "*microzymas*."

From this contradiction arose often violent discussions, in which **Pasteur** was unfair to **Béchamp**, which lasted for more than twenty years. Until his death, despite the formal proofs that were placed before his eyes, **Pasteur** stubbornly upheld the false principle of the atmospheric origin of ferments, denied their intra-organic origin, and supported the false dogma of the asepsis of the organism of living beings.

After him, his school, his students, and collaborators continued to support and defend the same errors, and today, more than three-quarters of a century after their development, his school still mounts a vigilant guard to preserve them inviolable and attacks any notion that might undermine them, thus engaging in a continuous struggle against the truth.

A clear proof of this can be found in the report that was drawn up by a Commission of the Biological Society in 1918, in an attempt to destroy the significance of **Portier's** experiments on "*symbionts*", a report that I have analysed on page 203 of this book. I must to say here that the total, definitive destruction of these dogmas is achieved by the new knowledge of the functions of the two elementary organelles which constitute living beings, animals and plants, and establish their entirely bacterial nature, of which the pasteurian dogmas are the negation.

The demonstration of the constitution of the fibrinferment by micrococcal granulations, the direct observation of their transformation into bacillary elements of the colibacillus, the demonstration of the constant existence, in the blood of higher animals, of micrococcal, bacillary, and even filamentous elements and of the germinative masses (platelets or hematoblasts and various leukocytes) of a culture of colibacillus, the demonstration of the presence of these micrococcal granulations of the colibacillus in all the liquids of the organism, normal or pathological, urine, milk, glandular secretions, etc., constitute the precise facts which establish the falsity of the pasteurian dogmas, and prohibit their defense in the future.

The demonstration of the nature of fibrinferment and its identification with *Bacterium coli* is the crucial fact that closes any discussion on the accuracy of Pasteur's dogmas of atmospheric panspermia and the asepsis of living beings.

By persisting in asserting the asepsis of living animal organisms, which are composed entirely of the dumbbell organelle, of a bacterial nature, and of *Bacterium coli*, **Pasteur** therefore committed the most considerable error that the biological sciences have ever known.

It is this error that the pasteurian School and the followers of pasteurian dogmas want to maintain and preserve in spite of everything, against the interests of the fight against human diseases, against the progress of science, but, on the other hand, for the sole benefit of particular material interests.

WHAT WAS THE EFFECT OF PASTORIAN FALSE DOGMAS

1. They have been harmful in distorting the judgment of the artisans of scientific progress and in directing their research in false directions. They have been harmful above all to the pasteurian School which, by supporting their accuracy, has forbidden itself, by this very fact, from being able to discover:

a) The bacterial nature of living beings.

b) The original source of *Bacterium coli* and its role in living beings.

c) The real nature of viruses and their original form which, for heterogeneous viruses, is not, in general, the bacterial form currently known.

d) The original source of heterogeneous viruses, which is most often a plant food.

e) The existence of autogenous viruses and diseases characterized by abnormal evolution of organic *Bacterium coli.*

f) The existence of spontaneous and autogenous diseases, due to the degeneration of the dumbbell organelle, tuberculosis and cancer, Koch's bacillus being the degenerated dumbbell organelle.

g) The identity between all forms of colibacillus, staphylococcus, streptococcus, pneumococcus, enterococcus, Bacillus lactis aerogenes, septic vibrio, tetanus bacillus, etc.

h) The bacterial nature of the ferments.

I will stop this enumeration, which I leave very incomplete.

2. Not only have pasteurian false dogmas hindered the progress of science, but they have also been the cause of many new errors, by directing research in the wrong directions. I will not list these errors here. The reader will find them in the article: *The Errors of Bacteriology.*

3. The assertion of the asepsis of living organisms, the ignorance of the existence of *Bacterium coli* and its functions in the animal organism, the assertion of the heterogeneous origin of the tuberculosis virus by **Villemin**, and later of Koch's bacillus, are responsible for the development of antibacterial chemotherapy, which has already caused a considerable number of victims, notably among tuberculosis patients and which has led to the misinterpretation of anticoli- bacillary therapy aimed at destroying colibacillus, a constituent agent of capital importance in animal organisms.

Let us add to this the creation of vaccines intended to vaccinate or combat these same colibacillus, streptococcus, staphylococcus, pneumococcus, (which are only one and the same element) and whose action is indispensable to our existence, and we will have an idea of the incoherence, the false and harmful direction in which pasteurian dogmas have led medical sciences.

4. Pasteurian false dogmas have prevented the search for the original source of heterogeneous viruses and, by this fact, have not allowed the pasteurian School to perfect its knowledge and to arrive, for example, at that of the multiplicity of diphtheria viruses;

these being specifically different, diphtheria toxoid, which comes from only one of them, cannot have an action against the others.

The same is true for the tetanus bacillus, whose original source I mistakenly located in the potato, and which, in reality, is the organic colibacillus, a fact which explains the hypothetically invoked presence of latent spores of the tetanus bacillus in the tissues. These spores are simply the colibacillary cocci which, in an oxygen-poor environment, evolve into the form of the tetanus bacillus. We will see, in the study of the tetanus bacillus, that being a form of the organic colibacillus, this fact implies the impossibility for the anti-tetanus serum to exert a therapeutic vaccinating action against tetanus.

Thus, by halting scientific progress, by the continuous accumulation of a mass of errors that they provoked, the false pasteurian dogmas have slowly led medicine, for three-quarters of a century in an extremely serious impasse, to a situation of incredible ignorance and incoherence, and bacteriology to decline.

* * *

It is certain that a strong reaction will greet the publication of this book. As was already the case with the publication of the second volume, I will be accused of being a ‘‘wrecker,’’ of "playing bowling" with the dogmas and principles of biology.

It is easy for me to respond to this accusation that I have only demolished what is false and harmful, and that I have reconstructed it by immediately replacing the false ideas with exact notions of capital importance that could never have been discovered without the demolition of false dogmas that I carried out. By this fact, I have opened up new avenues of research for science that will be very fruitful.

Moreover, in all the research presented in this book, what destroyed the false was the discovery of the exact fact and not a line of reasoning or a set of deductions; there was no demolition, there was only the automatic collapse of the false by the construction of the truth. The colibacillary function, by its construction alone, determined the automatic collapse of a series of errors, the main one being the dogma of the asepsis of living beings.

It is clear that the new notions I bring forward have the effect of a bomb falling in the middle of this swamp of errors, because they arrive all at once, in a science three-quarters of a century behind, instead of having been developed slowly, progressively, during this time.

But whose fault is it? Is it me, who began to develop them twenty years ago, who was immediately blacklisted and excommunicated, then deprived of all means of making them known in scientific collections? Is it me, who, on my second attempt, concerning

tuberculosis and Koch's bacillus, received a categorical refusal from the Academy of Medicine to publish my thesis, and to whom the Scientific Institute, defender of false pasteurian dogmas, sent delegates and its students to grossly insult me during a public conference intended to make the question known to the scientific world.

Original sin falls on the one who, against all scientific knowledge of the time, against all probability and truth, erected against the progress of science the insurmountable wall of the four dogmas incriminated here, especially those of atmospheric panspermia and the asepsis of living organisms, and wanted to persist obstinately in his error, despite the formal proof that the highest scientific authorities of the time placed before his eyes.

It falls on those who have wanted, for almost three-quarters of a century, to maintain all these gross errors by attacking any new notions capable of destroying them, and this exclusively to save material interests.

Those who committed such an error did not see that, by this method, they would one day arrive, through lack of continuous progress, at a state of scientific inferiority such that it would cause the collapse of their entire system. That day has arrived.

THE MAIN NEW CONCEPTS EXPOSED IN THIS BOOK

These new concepts are:

1. The destruction of the false notion of mitochondria and its replacement by the establishment of the real cellular structure and organization of the living matter of the cells and tissues of living beings of both kingdoms by the universal elementary constructing organelle, the dumbbell, which, although bacterial in nature, no longer has the form of a bacterium and is already clearly organized for the exercise of its function.

2. The extremely developed polymorphism of living matter.

3. The identification of the figured elements of the fibrin ferment (serozyme) with those of *Bacterium coli*, achieved by the continuation of the study of the mechanism of blood coagulation.

4. The bacterial function of living beings, which is the colibacillary function of higher animals. This function entails, in particular, the following four new notions:

a) The penetration of colibacillary cocci from the blood (fibrinferment) into the cytoplasm of glandular cells, which they only pass through to exit into secreted or excreted fluids, saliva, digestive juices, urine, milk, etc., of which they constitute the active fermentative elements.

b) Knowledge of the nature, origin, and mode of formation and evolution of leukocytes, which are the germinal masses of the colibacillary culture that actively vegetates in the blood of animals, and also of the origin, nature, and role of hematoblasts or platelets.

c) The identification with *Bacterium coli*, of which they are only variable forms, of the following so-called fixed bacterial species: staphylococcus, streptococcus, pneumococcus, enterococcus, Bacillus lactis aerogenes, septic vibrio, tetanus bacillus, aerobic septic bacillus of **Legros**, etc.

d) All diastases are the micrococcal form of the bacterial element which, along with the dumbbell organelle, constitutes living beings, animals and plants.

5. The identification of the septic vibrio with the colibacillus; knowledge of their nature and properties constitutes a formal demonstration of the inaccuracy of **Pasteur's** conclusions on putrefaction.

6. The autogenous origin and colibacillary nature of the tetanus bacillus and the spores that give rise to its culture in an anaerobic environment; its return to the bacillary form of the colibacillus by simple exposure to air of the tetanus toxin; the repercussions of these facts on the question of the effectiveness of antitetanus serum and toxoid.

7. The multiplicity of diphtheria viruses, their original source, their actual original form which is not, at the beginning, the bacterial form and the lack of correspondence between this multiplicity of viruses and the constitution of the antidiphtheria toxoid and serum, manufactured with only one of them, and of unknown original source.

8. The development of epidemics by ingestion of heterogeneous viruses which are often plant foods and not by transmission from one patient to another.

9. The rectification of the determination of the original source of several viruses: the septic vibrio, the tetanus bacillus, the smallpox-vaccinia, measles, scarlet fever, rabies, and plague viruses; and a very detailed understanding of the nature and original source of the childhood poliomyelitis virus.

10. The exact nature of the BCG vaccine and the demonstration of the impossibility of vaccinating against tuberculosis and cancer.

11. The cause of cancer: the degeneration of the cellular dumbbell organelle (the nuclear dumbbell in particular), giving it a power of multiplication that escapes the regulatory influence of blood plasma.

We will present these various concepts successively and succinctly, in order to give the reader a general understanding that will enable him to follow the questions presented more easily.

1. The false notion of mitochondria. The universal elementary organelle, the dumbbell, builder of the cytoplasmic and nuclear networks of the cells and tissues of living beings of the two kingdoms.

It is conclusively demonstrated in the first chapters of this book:

a) That mitochondria, as defined by **Regaud** and **Guilliermond**, for example, as free, mobile elements of highly variable shapes, without links between them, capable of evolving to form other elements, equipped with fermentative or catalytic power, have no real existence.

b) That they are only the scattered, shapeless debris of the dumbbell organelles and the cytoplasmic network disorganized, dislocated, and destroyed by the fixative.

c) That the fixatives, called mitochondrial, reputed by many authors, notably **Regaud** and **Guilliermond**, to rigorously preserve the mitochondria and the cytoplasmic structure, on the contrary completely destroy the latter, the constitution of which has remained unknown for this reason. Only formalin, used with caution, achieves sufficient preservation of the cytoplasm to allow its study.

The existence of mitochondria and their properties are therefore only the result of gross errors of observation.

d) That the cytoplasm and the nucleus of the cell are constituted by a network of dumbbells articulated together exclusively by their balls, a network in which all the empty spaces communicating with each other form a single cytoplasmic cavity.

e) That the universal elementary organelle, builder of the cells and tissues of living beings of the two kingdoms, is the dumbbell and not, as **A. Béchamp** claimed, the free, mobile granular element, which he called microzyma.

The dumbbell organelle, consisting of a rod of variable length, bearing a ball or granulation at each end, is already an element organized for the function it fulfills, these balls being intended for its articulation with other similar elements to form either networks (cytoplasmic and nuclear networks, myelin sheath network), or filaments when the dumbbells are articulated only end to end; the type of these is the neurofibril of the axis -cylinder of the nerves or the neurofibril of the nerve centers, presenting varicosities each of which is the assembly of two dumbbell balls.

Throughout the entire scale of living beings in both kingdoms, down to the lowest forms, the hyphomycetes, the articulation of the dumbbells by their balls remains rigorously constant.

In a cell, the mass of the dumbbells constituting the cytoplasmic network therefore represents the entirety of the living matter in the cytoplasm.

f) The universal dumbbell organelle therefore has the crucially important function of building the structure and organization of the living matter of organized living beings.

2. The extremely developed polymorphism of living matter.

The polymorphism of living matter is the property that has allowed it to evolve from its primitive micrococcal bacterial form to the form of higher animals. This property is demonstrated by:

a) The transformation of the micrococcal bacterial form into the bacillary form and, more generally, the transformation of one bacterial form into several others and the change in its biological properties through evolution or degeneration.

b) The transformation of a bacterial culture into a hyphomycete culture.

c) The transformation of a hyphomycete, a *Botrytis*, for example, into bacterial elements, if it is cultivated in broth at 30-37°C, or into an organized fungus of the genus Pézyze (*Sclerotinia fuckeliana*) if its sclerotia are buried under a one-centimeter layer of damp sand.

e) The transformation of a hyphomycete from one conidial form to another.

d) The transformation of the living matter of an animal or plant into multiple conidial forms of hyphomycetes.

f) The transformation of the living matter of a plant in a state of suffering into the production of such varied forms that we currently consider to be cryptogamic diseases due to heterogeneous germs, whereas they are autogenous diseases (Ustilaginea, Uredinae, Spheriaceae, etc.).

g) The change in the biological properties of the dumbbell-building organelle by degeneration (tuberculosis, cancer). The extreme polymorphism of living matter is demonstrated in this study by other multiple very convincing examples.

3. Identification of the active figurative elements of the fibrinferment (serozyme) with those of the Bacterium coli.

This identification, of considerable importance, was made during research aimed at determining the causes of blood clotting. The main facts that led to it were already set out in 1926 in the first volume of this book.

It has been accepted that the body called thrombin, which causes blood clotting, is formed by the union of two ferments, one called serozyme, existing in the blood, the other called cytozyme, existing in the tissues according to some authors, but also existing in the blood according to **Bordet** and **Delange**.

I have demonstrated that the fibrinferment is constituted exclusively by serozyme and that, on the other hand, **Bordet's** cytozyme, being constituted by platelets, does not exist and is confused with serozyme; finally, that thrombin is the acid group of blood lecithins (glycerophosphoric acid and fatty acids) which, detached by the hydrolyzing action of serozyme, attaches to fibrinogen (protein-soda soap complex) and coagulates it by making the soda soap acidic.

I have also demonstrated that the active agents of serozyme are granules of approximately 0.3 to 0.8 microns and that the addition of gelatinous tricalcium phosphate to oxalated blood plasma delays its spontaneous coagulation because it carries with it a portion of the active serozyme granules.

The key fact observed was the spontaneous formation in oxalated plasma or in fibrinogen solutions treated with tricalcium phosphate, of light white flakes which, upon microscopic examination, were found to consist of clusters of mycelial filaments and filaments made up of chains of bacilli that have the appearance and characteristics of colibacillus.

When inoculated in broth, these flakes give rise to a culture of colibacillus. Other observations showed that the cocci, which constitute the serozyme, actively multiply in oxalated plasma and in fibrinogen solutions, then form germinative masses that give rise to colibacillus filaments.

Direct examination of the elements that constitute the white layer that covers the blood cells of centrifuged oxalated blood shows that it is composed entirely of all the elements of the colibacillary culture of the blood: cocci, short and long bacilli, long filaments, platelets or hematoblasts, and leukocytes, these being the germinal masses of the culture, the mononuclear cells being the most evolved.

A portion of the white layer inoculated in broth gives rise to a colibacillary culture. The identification of the fibrinferment with *Bacterium coli* was thus established. It is the micrococcal elements of the latter that constitute the fibrinferment.

4. The bacterial function of living beings, colibacillary function in higher animals.

The demonstration of the constitution of the fibrinferment of the blood of higher animals by the micrococcal granulations of the colibacillus, which are those that **Béchamp** had called microzymas, establishes in animals the existence of the capital colibacillary function or, better, of the general bacterial function of living beings, attested by the existence, in every animal or plant individual, of these micrococcal granulations endowed with fermentative power. This is exercised exclusively by these micrococcal granulations.

Thus, in every living being, it is a bacterial species, the colibacillus in higher animals, which fulfills the crucial function of carrying out all the chemical transformations that take place in the organism. It performs this function in the blood and in the cells and also in the intestine through the digestive juices whose active elements are the colibacillus cocci from the blood that they have carried with them.

The normal habitat of the colibacillus is the blood; its micrococcal granulation, with a size of 0.3 to 0.7 or 0.8 microns, is extremely mobile and driven by Brownian motion. *It constitutes the second elementary constituent organelle of the organism*, very different in form and properties from the dumbbell-building organelle.

The study of the function of the colibacillus has brought to light four important new notions:

A) The penetration of colibacillary cocci from the blood into the cytoplasmic cavity of glandular cells, which they only pass through to exit with the secreted or excreted liquids, urine, milk, saliva, digestive juices, various glandular secretions, pathological liquids, etc., and of which they are the active fermentative elements.

The colibacillary cocci, although its presence in the cell can be demonstrated, are therefore not part of its constitution; they are foreign to it and only pass through it, their normal habitat being the blood.

B) Knowledge of the nature, origin, and mode of formation and evolution of leukocytes, already indicated in the first volume of this work. They are the germinative masses of the *Bacterium coli* culture; the granules they contain are the colibacillary cocci; the most

evolved are the mononuclear ones, and particularly those whose basophilic nucleus occupies the entire space.

They do not have two qualities: it is exclusively the quantity of basophilic chromatic matter they contain that determines their designation as acidophiles, neutrophils, or basophils, and it is exclusively through the agglomeration of basophilic elements that they acquire this chromatic matter.

C) The identification with *Bacterium coli*, of which they are only variable forms, of the following so-called bacterial species: staphylococcus, streptococcus, pneumococcus, enterococcus, aerogenous *Bacillus lactis,* septic vibrio, tetanus bacillus, etc. This fact will be demonstrated by studying these so-called bacterial species.

D) All diastases are the micrococcal form of the bacterial element which, with the dumbbell organelle, constitutes living beings, animals and plants.

5. The identification of the septic vibrio with the colibacillus; this identification, together with that of the fibrinferment with the colibacillus, constitutes a formal demonstration of the inaccuracy of Pasteur's conclusions on putrefaction.

Pasteur concluded from his research:

1. That the animal organism is closed, under ordinary conditions, to the penetration of germs from inferior beings, and, *a fortiori*, that it is aseptic;

2. That the putrefaction of corpses is due to the penetration, into the interior of the body, of germs that soil its external surface and of germs that are contained, fully formed, in the interior of the intestinal tract and which are notably, according to bacteriologists, the colibacillus and the septic vibrio.

The identification of the septic vibrio with the colibacillus results from its shape, its properties, and its development in the intestine and, above all, from its normal development in blood extracted aseptically and preserved away from atmospheric oxygen.

The fact of the normal presence of the colibacillus, that is to say of the septic vibrio, in all parts of the organism and the function of capital importance that it fulfills there, are sufficient to demonstrate peremptorily, without it being necessary to insist, the falsity of **Pasteur's** conclusions on putrefaction.

6. The autogenous origin and colibacillary nature of the tetanus bacillus and the spores that give rise to its culture in an anaerobic environment; its return to the bacillary form of the colibacillus, by simple exposure to air of the tetanus toxin. The repercussions of these facts on the question of the effectiveness of antitetanus serum.

In the first volume of this work, it has already been demonstrated that tetanus toxin, which is the product of filtration through a porcelain candle of anaerobic cultures of tetanus bacillus, owes its activity to the granules of 0.3 to 0.8 microns that it contains; it is taught that it can only be preserved away from air.

In reality, it does not alter when it is brought into contact with air, *but it evolves.* The granules it contains group into germinative masses that give rise to long filaments that segment into bacillary elements; they are photographed in Figure 6 of Plate 206 of the first volume of this work.

At this time, I made a false determination of the original source of the tetanus bacillus, concluding that this source was the carrot.

When I had identified the fibrinferment with the colibacillus, I made a new study of the bacillary form resulting from the evolution of the tetanus toxin in the air, and I thus determined that it was the normal form and culture of the colibacillus that reappeared; this fact therefore established the autogenous nature of the tetanus bacillus and tetanus.

At the same time, two other identifications, that of the scarlet fever virus and that of the rabies virus, turned out to be false because they resulted from a comparison of the same conidial form of the carrot that had misled me. I demonstrated in this book, in Chapter XII:

1. That the development of the tetanus bacillus is autogenous and results from an abnormal evolution of the organic colibacillus in an anaerobic environment; that this fact explains the tetanus caused by injections of quinine salts, or by surgical cutting in castrated animals, or that which develops after aseptic surgical operations;

2. That the autogenous tetanus bacillus of an animal becomes heterogeneous for another animal, the horse, for example, into which it is injected and causes the formation of an antitoxin in its blood, this being active only against the antigen strain of toxin and only against the experimental tetanus caused by it in animals of a different species;

3. That the fact of the autogenous origin of tetanus eliminates any possibility of vaccination against it, which is also peremptorily demonstrated by these two facts: that a first attack of tetanus does not vaccinate and that the blood of a man cured of tetanus does not contain antitoxin (Vincenzi, 1893). *This establishes the absolute, inevitable ineffectiveness of antitetanus serum and toxoid.*

7. The multiplicity of diphtheria viruses; their original source; their actual original form; the consequences of this fact.

In the first volume of this book, it was stated that barley is the original source of the diphtheria virus. Subsequently, I conducted a much more comprehensive study which, by confirming that one of the sources of this virus is barley, demonstrated that there are multiple viruses, numbering six and more. Until now, it was believed that the diphtheria false membrane is made up of an exudate secreted by the pharyngeal, laryngeal, and tracheal mucous membranes.

Studying this false membrane, I demonstrated that it is always made up entirely and, beyond the slightest doubt, of a hyphomycete, which is often *Cladosporium herbarum.*

By inoculating the conidia of *Cladosporium* from barley into broth, obtained either by culturing the flour of the burnt grain or by culturing the spores of the smut that invades the ears (an autogenous disease), I obtained the most beautiful cultures of diphtheria bacillus that can be seen (pl. 64, 3rd vol.).

Photographs of the mycelial colonies that develop in the culture show that the diphtheria bacillary elements are born on the already developed mycelium of the *Cladosporium*, that is, are only a secondary product of it, the real and original form of the virus being the spore and the mycelium of the *Cladosporium* that forms the false membrane.

I also obtained cultures of typical diphtheria bacillus with *Cladosporium* and even directly with healthy wheat, barley, and rye flour. By inoculating these cultures onto the bare epidermis of the rabbit's ear, I obtained false diphtheria membranes containing countless quantities of diphtheria bacilli identical in shape to those of human diphtheria membranes.

I was also able to transform and directly bring back, in vitro, a verified culture of diphtheria bacillus into the *Cladosporium* form. Another species of hyphomycete, *Cytromyces* of barley, wheat, and rye, also gives rise to diphtheria-type cultures and, on the bare epidermis of the rabbit's ear, to a false membrane containing diphtheria bacilli of typical shape.

Since there are multiple diphtheria viruses, and the toxoid used for vaccination is obtained from only one diphtheria virus, it can, if it confers protection, only do so against this virus, and it is inactive against the others.

The same criticism is leveled at the therapeutic action of anti-diphtheria serum. Proof of this inactivity of diphtheria toxoid lies in the fact that supposedly vaccinated subjects

contract diphtheria just as easily as unvaccinated subjects; the allegation that they have less severe diphtheria is inaccurate.

These facts demonstrate, to say the least, how lightly the public authorities agreed to make vaccinations compulsory instead of leaving them optional.

The nature of the original source of diphtheria viruses and the very constant location of the initial diphtheria lesion, the pharynx, with its false membrane, demonstrate peremptorily that the notion of infection by contagion is false and that it occurs by ingestion, with cases of contagion by the patient being rare and exceptional.

It is cereal flour, especially when infected with spores of Cladosporium herbarum or cereal smut or other cryptogamic diseases, which develops the diphtheria false membrane on the tonsils and pharynx.

This fact conclusively demonstrates that the vaccination method of prevention is not the right method of choice. It exposes us to the dangers of introducing virulent microbes into the body. The method of choice, that of the future, consists of knowing exactly the nature of the virus, its original source, and, knowing this source, avoiding absorbing it or destroying the virus there by heat.

8. The development of epidemics occurs through the ingestion of heterogeneous viruses, which are most often plant foods, and not through transmission from one patient to another.

For fifty to sixty years, epidemiology has not made the slightest progress. However, during this period, numerous serious epidemics of diphtheria, eruptive fevers, cerebrospinal meningitis, infantile poliomyelitis, plague, etc. have occurred. Reading the reports concerning the investigations carried out into these epidemics to find their cause shows that the cause remains completely unknown and that these investigations do not reveal the slightest path, the slightest clue that could provide a direction for future research.

This state of stagnation in science stems from the same causes already indicated: the influence of the two harmful dogmas of atmospheric panspermia and the asepsis of the organism of living beings, which have distorted the mind and reduced to impotence the reasoning of the artisans of scientific progress.

Convinced of the falsity of these dogmas, I was able, in the first volume of this work, to establish the bacterial and mycelial nature of living organisms, a fundamental notion, from which quickly emerged the following fundamental principles, which will dominate future progress in the fight against diseases of humans and animals.

1. The original source of pathogenic viruses of diseases is exclusively the organism of living beings.

2. Some viruses are autogenous and develop spontaneously in the patient's own organism.

3. The viruses of heterogeneous diseases are the living matter of organized beings and, in general, of those that serve as food for animals and humans.

These principles led me to establish means of study that would allow me to achieve knowledge of the original source of viruses.

It is quite obvious that those who base their reasoning on the aforementioned false dogmas could not achieve such results since these dogmas constitute the formal negation of the principles I have just set out.

We have come to know a bacterial form of the viruses of diphtheria, typhoid fever, plague, etc. What use has this been from an epidemiological point of view? Nothing, or almost nothing.

We can, however, succeed in completely and clearly elucidating the causes of the development of an epidemic. Here, for example, is how I did it for diphtheria:

1st step. The development of the virus on the tonsil in the first place implies that it is brought there by food.

2nd step. Applying the principle: *Animals and plants are the sources of pathogenic viruses*, I transformed the diphtheria cultures into *Penicilium*, then I also transformed the living matter of the various foods given to children from the age of six months to three years into *Penicilium*, because they provide the highest proportion of diphtheria cases. Comparing these various *Penicilium* with each other, I saw that it was those from wheat and barley that most resembled those from diphtheria cultures. The original source was thus found.

3rd step. By studying the false membranes from cases of croup and other cases of diphtheria, I saw that they were all made up of the mycelium and conidia of various hyphomycetes, one of which is *Cladosporium herbarum*. The original virus, which is not *Laefler's bacillus*, was thus discovered.

4th step. I caused the formation of false diphtheria membranes on the bare epidermis of the rabbit's ear, by inoculating it with conidia of the conidial forms *Cladosporium* and *Citromyces* from wheat, barley, and rye, and I verified that these same conidia inoculated in broth developed either directly into diphtheria bacilli, or mycelial colonies whose branches give rise to characteristic diphtheria bacilli.

It was thus established that the conidia of *Cladosporium* and *Citromyces* of wheat, barley, and rye are the original virus and that *Læsler's bacillus* is only a secondary form of the virus, subsequent to the formation of the mycelium.

Once these four stages have been completed, the mode of development of a diphtheria epidemic has become clear: It is by ingestion of cereal flours contaminated by conidia of their cryptogamic diseases that diphtheria is contracted, and an epidemic will develop in a single district of a city if this single district has received contaminated flours or receives milk contaminated by stable manure whose straw and empty ears contained numerous conidia of cryptogamic diseases.

All epidemiology is contained in this example: it consists of determining the name of the animal or plant carrying the virus and applying appropriate food hygiene measures designed to prevent the ingestion of the virus or to destroy its vitality before ingesting it.

This mode of epidemic development is so clear and apparent that, simply by studying the documents relating to the 1927 polio epidemic in Romania, I was able to see that polio is a diphtheria caused by an extremely virulent conidia of cereals infecting the pharynx and nasal cavities and that the sure, rapid way to radically stop a polio epidemic is to completely remove from the children's diet, from the first cases, cereal flours of all kinds, as well as cow's milk, and replace them with porridges of vegetables, potatoes, lentils, peas, carrots, etc.

In summary, the dogma of contagion is false: It is by ingestion of the virus food and not by contagion that a heterogeneous virus disease is contracted.

Let us add that the fight against epidemics by disinfecting walls, clothing, and bedding is completely useless, the virus not being there, but in the food.

The fight through vaccinations can have no effect on an epidemic, the vaccines currently used being totally ineffective and, moreover, harmful. Their only effect is to communicate to the vaccinated the chronic phase of a disease, which increases their morbidity.

9. The rectification of the determination of the original source of several viruses: the septic vibrio, the tetanus bacillus, the smallpox/vaccinia, measles, scarlet fever, and rabies viruses; and a very close understanding of the infantile poliomyelitis virus.

Since the birth of bacteriological science in the middle of the last century, the microbe has been considered the usual agent of contagious diseases. The pathogenic virus is still generally considered today as a microbial culture that spreads in the external environment, without its habitat ever having been precisely determined.

The research presented in the first volume of this work has brought to light and demonstrated the following new fundamental principles:

1. Bacteria can easily be transformed into hyphomycetes and, conversely, hyphomycetes into bacterial elements, by simply changing the culture medium.

2. All living beings, animals and plants, have a mycelial and bacterial anatomical constitution.

3. The living matter of animals and plants, aseptically extracted from the body and cultivated in various nutrient media, gives rise either to bacterial elements of very diverse shapes and sizes, or to mycelial elements that can affect multiple and very diverse conidial types, for the same animal and plant species.

4. All bacteria, all hyphomycetes found in the soil, on decomposing materials, on the most diverse objects, all viruses, come from the animal and plant organisms that gave rise to them and spread them outside, either through their excrement or through their decomposition after their death.

5. Viruses of human and animal diseases are of two categories: Some are autogenous and due to an alteration of the bacterial or mycelial elements of the organism. *The others, heterogeneous, are almost exclusively the food materials of humans and animals.*

From these principles, the method of identifying viruses was derived, that is to say, the search for their original source, which consists of:

1. Transforming the isolated virus into various bacterial and mycelial forms.

2. Comparing these with the bacterial and mycelial forms of food, plant, and animal substances.

It was by making this comparison that I was able to recognize the *Penicilium* of barley in the *Penicilium* obtained by culturing a false diphtheria membrane.

And it was with this comparison in mind that I studied and photographed some of the most common conidial forms of most plant and animal species that are human food. These photographs are included in the first volume of this work, where the method is explained in detail.

The comparison of *Penicilium* forms is extremely delicate and difficult, except in certain cases, such as that of *Penicilium italicum*, for example, whose exclusive habitat is the lemon, and whose appearance makes recognition easy in the *Penicilium* obtained from *Micrococcus melittensis* (Malta fever).

It is necessary to carry out research on as many bacterial and conidial forms as possible.

* * *

VACCINIA, SMALLPOX, CHICKENPOX, MEASLES, AND SCARLET FEVER VIRUS

The method I have just described was used to search for the smallpox-vaccine virus and its original source.

I found and photographed the virus quite easily, but I have not yet succeeded in discovering its original source.

Until now, this virus has been considered a filtering virus because no one has ever succeeded in isolating a cultivable bacterial element that, upon inoculation, reproduces the vaccinal pustule and because inoculation of the lymph from vaccinal pustules, which is said to contain no formed elements, develops it.

In my research prior to 1926, and then in others that followed, I had obtained, by culturing the vaccinal lymph, certain forms of hyphomycetes that I had identified with that of the potato. This was a mistake. For two years, I tried unsuccessfully to extract the smallpox and vaccinia viruses from the potato, without success.

Although I have not yet been able to put a certain name to the original source of smallpox and vaccinia, I have nevertheless determined and isolated the virus without the slightest possible doubt, because it abounds in the vaccinal pustule, in the form of a hyphomycete of the probable *Spicaria* type, whose mycelium and conidia are, in innumerable quantities, the elements that produce the vaccinal pustule and completely destroy the dermis.

The conidia are oval and isolated at the end of a small conidiophore. Those of the variola pustule have a slightly longer conidiophore. A photograph of the mycelium and its conidia can be found in plate 70 for smallpox, and in plates 71 and 72 for cowpox.

During a new study of diphtheria, my attention was drawn to the simultaneous development, in certain patients, of measles and diphtheria, or of scarlet fever and diphtheria,

and then also by the simultaneous development of measles and scarlet fever without diphtheria, facts which demonstrated to me that these diseases are all due to the ingestion of the same category of foods, cereal flours. Measles and scarlet fever can also develop after the onset of diphtheria, during its progression.

But how could this be understood, since diphtheria is already caused by these same foods? This is where the capital importance of the studies presented in the first and third volumes of this work, on the polymorphism of living matter, appears.

A grain of wheat, sanitized and ground, then cultured on agar, can give rise to several hyphomycetes of different conidial forms, for example, a *Cladosporium herbarum* form, a *Sporothricum* form, a *Spicaria* form, a *Penicilium* form, and another *Aspergillus*; the conidia of cereal smut, which almost always contaminate grain and flour, also give rise to this multiplicity of conidial forms when cultured on agar or other solid media. This same phenomenon occurs in the pharynx when fragments of cereal porridge remain lodged there and evolve into hyphomycetes. Consequently, a single species, wheat flour or barley flour, can give rise to a *Cladosporium* conidial form that will develop diphtheria and another conidial form that will develop measles or scarlet fever.

We have seen previously that at least two different conidial forms of the same cereal, the Cladosporium and Cytromyces forms, for example, can give rise to a diphtheria false membrane. It is also possible that, in the false membrane itself, the Cladosporium conidial form transforms, during the course of the disease, into another conidial form, which causes the development of measles or scarlet fever.

By studying the bacterial forms that I obtained in scarlet fever and comparing them to that of rye, it appeared to me that it could be caused by a conidial form of rye.

These indications require precise control and determinism, which is difficult to achieve, but very possible and is currently being studied. It thus appears that one of the most dangerous sources of viruses of heterogeneous diseases of humans and animals is the living matter of food grains.

I recall that, during the rapid attempt at a general identification of pathogenic viruses, the results of which are presented in the first volume, I was led to conclude that wheat is the original source of paratyphoid virus A, rye that of paratyphoid virus B, corn that of typhoid fever, and oats that of exanthematic typhus. Ongoing studies will soon, I hope, provide clarification on these various points.

* * *

It therefore appears that, without this knowledge of the multiplicity of conidial forms that the living matter of an organized being can take, the search for the original source

of viruses, their nature, and their real form would have led to incomprehensible results and inextricable complications.

It also follows from this presentation that the identifications set out in the first volume on measles, scarlet fever, smallpox, and chickenpox are inaccurate. The error stems largely from the extreme difficulty of differentiating the Penicilium forms of the various plant species. On the other hand, I have not made sufficient use of the study of other conidial forms and the study of bacterial forms. This is due to the enormous work I had to accomplish, with very minimal help, on an entirely new and pitfall-ridden subject, made extremely difficult by the false dogmas of bacteriology.

As early as 1924, I succeeded in establishing this research method, which allows us to understand the original source of viruses and their exact form. From the very beginning, I obtained accurate determinations for tuberculosis, cancer, colibacillus, staphylococcus, pneumococcus, diphtheria, Malta fever, and trichophytosis.

Above all, I wanted to make a rapid effort to identify all the main pathogenic viruses. It is certain that a certain number of the determinations are inaccurate, but the fact is of little importance, as all are intended for much more extensive subsequent testing. The results obtained for the accurate determinations indicated above are sufficient in themselves to justify the method and demonstrate its capital importance.

From now on, the method has provided the following new knowledge:

1. Knowledge of the original source of the colibacillus, its crucial physiological functions, the identity between colibacillus, staphylococcus, streptococcus, pneumococcus, enterococcus, septic vibrio, aerogenous Bacillus lactis, and other bacterial forms, sheds light on a whole category of autogenous human diseases: colibacillus diseases, erysipelas, pneumonia, the nature and origin of suppurations, etc.

2. Knowledge of the original source of diphtheria viruses, their multiplicity, explaining and proving the inevitable ineffectiveness of antidiphtheria serum and toxoid.

3. Knowledge of the original source of the tetanus bacillus and the autogenous nature of tetanus, leading to the inevitable ineffectiveness of antitetanus serum and toxoid.

4. Knowledge of the original source of the tuberculosis and cancer viruses and the autogenous nature of these diseases.

5. Knowledge of the vaccinia and smallpox viruses: only the determination of the original source of these two viruses is missing, knowledge of which destroys the inexact notion of filtering viruses.

6. Knowledge of the original source of micrococcus melittensis, which is the living matter of the different species of Aurantiaceae.

7. Sufficiently detailed knowledge of the original source of the poliomyelitis virus (food grains) and the means of stopping epidemics of this serious disease at their onset.

10. The exact nature of the B.C.G vaccine and the demonstration of the impossibility of vaccinating against tuberculosis and cancer.

It is demonstrated, in the second volume of this work, that Koch's bacillus is the dumbbell organelle that constructs the cells and tissues of the animal carrying tuberculosis and which has acquired, through degeneration, the tuberculizing property.

The facts presented in this third volume show that the organic colibacilli of the various animal species are specifically different and that, if the colibacillus of one species is not virulent for it, it is virulent for another and causes heterogeneous colibacillosis and the state of anaphylaxis. This leads to the certainty that the dumbbell organelle, nonvirulent for its original organism, is or can be virulent for the organism of another species.

BCG is the bovine tubercle bacillus, which has become non-virulent for cattle by cultivation in a bile medium, that is, the tuberculizing bovine dumbbell organelle, which *has become normal and non-tuberculous by this culture process.* It is therefore normal that BCG, the normal dumbbell organelle of cattle, is not virulent for this animal; but from this fact, we must not conclude that it is harmless to humans, because, this time, the dumbbell organelle of the latter is specifically different from the BCG dumbbell; on the contrary, like the colibacillus of cattle, the BCG dumbbell must be virulent for humans.

The inoculation of the normal dumbbell organelle of cattle into humans is an act equivalent to the inoculation of the organic colibacillus of cattle into humans, which is constantly virulent for them.

The specific difference that exists between the bovine Koch bacillus and the human bacillus on the one hand, and between the human dumbbell organelle and the bovine dumbbell organelle on the other, leads to the following consequences:

1. A bovine Koch bacillus cannot vaccinate against the human bacillus because it is specifically different.

2. A bovine Koch bacillus, which has permanently lost its tuberculizing property (B.C.G.) and thus become an apparently normal dumbbell organelle, has become specifically different from the bovine tuberculizing bacillus.

A vaccine can only be effective if it is the virus itself, whose *virulence alone is diminished by an artificial process*. If it has completely lost its specific virulence, it thus loses all vaccinating power.

This is a formal, well-established, and well-known principle that is not realized in the development of BCG. This vaccine, originally a bovine tuberculosis bacillus, can therefore no longer vaccinate, even the ox, against its own tuberculosis, precisely because it is no longer tuberculous, and because, as a result, it can no longer vaccinate against a property that it does not possess.

3. BCG, which is the tuberculizing bovine halter organelle, returned to the state of the apparently normal and non-tuberculating ox halter organelle, is not virulent for the ox because of this quality; but this fact does not allow us to conclude that it is non-virulent for humans, whose halter organelle is specifically different from that of the ox.

11. The cause of cancer: the degeneration of the cellular dumbbell organelle (the nuclear dumbbell in particular), giving it a power of multiplication that escapes the regulatory influence of blood plasma.

Observation of the development of cancerous tissue shows that it takes place both by the formation of cellular elements, and by the formation of fibrillar elements which, grouped in large parallel bundles, form the horny vortices and globes. Each fiber is a filament of dumbbells assembled end to end by their balls, and emanates, with a group of others, from an embryonic cell.

As for the cells, I have not noticed that they are formed by kinetic division of the nucleus. I have never noticed in a nucleus of a cancerous cell, any real image of metaphase or anaphase. The cell nuclei appear to be born at the end of the dumbbell filaments, and, once formed, are surrounded by cytoplasm. These nuclei grow considerably, exactly like the epithelioid cells of tuberculous tissue, then eventually become caseous and necrotic.

In the globocellular type of sarcoma, the round cells appear to be nuclei without cytoplasm, which are initially small embryonic cells consisting exclusively of dumbbells. Having reached a certain size, these large round nuclei become emission centers for dumbbell filaments that enter the lymphatic ducts and there in turn develop new spherical nuclei.

In short, whether it is cellular development or the development of fibrillar elements that affect the form of connective bundles, it is always an accelerated, unlimited multiplication of the dumbbell organelle that forms the mass of the cancerous tissue.

It is therefore the dumbbell organelle that, as with tuberculous tissue, forms the cancerous tissue and is, in short, the cancer-causing bacillus, the cancer-causing virus, exactly homologous to the tuberculous dumbbell, which is Koch's bacillus, or tuberculosis virus.

As with tuberculosis, the fact that the cancer agent is the dumbbell organelle, which has become cancerous through degeneration, shows us that we cannot vaccinate against cancer because we cannot vaccinate against a normal constituent of the organism, even if degenerated, and because it is not a question of vaccinating against an element, but only against the cause of its degeneration.

Since we know that we can only immunize against a bacterial element by inoculating this element, which has retained its virulence to a certain extent, we understand that such an inoculation could not modify the cause that determines the degeneration of the dumbbell into a tubercular or cancerous virus, or which could in any way modify its power of multiplication.

Only a modification of the constitution of the environment could have such an effect and succeed in canceling out, as **Calmette** and **Guérin** achieved in vitro for BCG, the tubercular or cancer-causing power of the dumbbell organelle.

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FREEDOM OF SCIENTIFIC OPINION AND DISCUSSION IN FRANCE

I have deemed it necessary to now make known how, in France, a disinterested worker is treated, whose sole aim is to advance the fight against human diseases, and the abominable methods employed either to prevent him from sharing the new concepts he acquires, or to make it impossible for him to continue his studies.

The presentation of the new concepts just reviewed, as well as the content of the first and second volumes of this work, demonstrate that the research I was pursuing was of capital importance for the progress of medicine and the fight against disease.

It is precisely to prevent the achievement of this progress that a scientific institute wanted to suppress the results of my research.

A complete account of the struggle that was waged against the disclosure of the results of my scientific research can be found in the article in this book entitled: *The Strangling of Freedom of Opinion and Scientific Discussion in France* (page 207).

The person who initiated this struggle and established the strangulation of freedom of opinion and scientific discussion in France was Professor **Roux**, Director of the Pasteur Institute, assisted by his colleagues and by a permanent secretary of the Academy of

Sciences, a non-biologist and incompetent. What motive prompted Professor **Roux** to commit such serious and reprehensible acts?

This motive is a guiding principle that has guided the pasteurian School for three quarters of a century: the stifling of any new notion that demonstrates the falsity of pasteurian dogmas; more precisely, it is the publication of the first volume of the work entitled: *Constitution of Animal and Plant Organisms. Causes of the Diseases That Affect Them.* The stifling of the remarkable results of **Béchamp's** work was the first act in this fight against the truth. The new notions contained in the book indicated above were:

1. The demonstration of the inaccuracy of pasteurian dogmas;

2. The demonstration of the mycobacterial nature of living beings;

3. The demonstration of the existence of autogenous virus formation in the animal organism;

4. The demonstration of the mode of evolution of bacteria and the inaccuracy of the dogma of bacterial monomorphism;

5. The demonstration of the bacterial nature of the leukocytes of the animal organism;

6. The demonstration of the animal or plant origin of all disease viruses;

7. The demonstration of the process for achieving knowledge of the original source of viruses, particularly heterogeneous viruses;

8. The knowledge of the original source of certain viruses, particularly that of diphtheria, which is cereal flour.

Since these facts were likely to cause the collapse of dogmatic bacteriology and the shaky scaffolding of false notions imagined and defended by the pasteurian School, Professor **Roux** judged that they must be stifled at all costs and their disclosure prevented.

Among us, there were many men who defended the freedom of opinion of the political press. In the Academies and Learned Societies, not a single man stood up to defend freedom of opinion, discussion, and scientific publication.

In 1936, during the publication of the second volume of this work, in which I established the autogenous and spontaneous nature of tuberculosis and the nature and origin of Koch's bacillus, the dumbbell organelle that constructs anatomical elements adulterated by degeneration, the same comedy was repeated. Members of the pasteurian School forgot themselves to the point of coming, with a group of students, to grossly insult me during a public lecture I was giving at the Museum of Natural History on the nature and origin of Koch's bacillus and tuberculosis.

Since that time, a watchword has been passed everywhere, in medical journals, learned societies, and the Commissions of the Fund for Scientific Research, to workers, even in

laboratories, to prevent any new publication and to sterilize my efforts by refusing me the allocation of technical assistance and any grant, either for research or to help me with my publications.

This continual antagonism, which aimed to make it impossible for me to continue my work, which was considered dangerous for the existence of the false dogmas of official bacteriology and even more so for considerable material interests, had the result of making me a worker who was shunned like a plague victim or an excommunicated person a few centuries ago and of making it impossible for me to find collaborators.

To carry out my research, I had to resort to the help of inexperienced hands whom I had to educate. The results presented in this book prove that the attempt to suppress the results of my research was mainly motivated by the fear that they would harm considerable material interests.

These facts had to be known to the medical world, which will now judge.

* * *

I conclude by expressing my thanks here:

To the **Colleagues at the Museum**, whose help has been invaluable to me; To **Mrs. Dindault**, assistant at the Museum, whose skill and technical competence as a histologist have been of great help to my research; To the many doctors who have supported and helped me with their encouragement, sympathy, knowledge, and advice; To Messrs. **Blondel La Rougery**, publishers, and to Mr. **V. Richou**, Director General of Editions de l'Ouest, in Angers, whose assistance has been invaluable to me in the publication of this volume.

CHAPTER ONE

STUDY OF THE CHONDRIOME AND THE MICROSCOPIC ANATOMY OF THE CYTOPLASM OF PLANT CELLS

This chapter will include:

I. A presentation of current knowledge on the constitution of the cytoplasm.

II. A study of the technique of cytological research.

III. The study of the chondriome and the structure of the cytoplasm of plant cells.

IV. The study of chloroplasts.

V. The study of amyloplasts.

I. PRESENTATION OF CURRENT KNOWLEDGE ON THE CONSTITUTION OF THE CYTOPLASM

The first precise knowledge of the structure of the cytoplasm seems to be attributed to **Küpfer**, who, in 1875, reported in the liver cell of batrachians the existence of a protoplasmic reticulum extending throughout the cell, from the nucleus to the cell membrane, forming larger meshes at the periphery than around the nucleus. These observations were made using the osmic acid fixation technique, which **Küpfer** was the first to use (20).

On the other hand, **Ewald** and **Kühne** (19) demonstrated in 1877 the existence, in the myelin sheath of nerve fibers, of a network of fibrils that they called the neurokeratin network because, according to them, it was formed by a horny substance not attacked by digestive enzymes. In the same year (1877), **Lanterman** (37) described, in just a few lines, the network of the myelin sheath that bears his name and which should rather bear those of **Ewald** and **Kühne**, whose description was much more complete: (see p. 79)

This network must be identified with the cytoplasmic network of the cell; indeed, the **Ewald** and **Kühne** neurokeratin network of nerve fibers must be considered as the extension, at the periphery of these fibers, of the pericellular Golgi network, a network which is, in the original nerve cell from which the cylinder-axis of the fiber originates, the equivalent of the cytoplasmic network of cells in other organs (see page 95).

The results of **Küpfer's** observations were confirmed and completed by **Heidenhain** (1880), **Flemming** (1882), and **Eugénie Koiransky** (1904).

At the same time, **Klein** (1879) described this same cytoplasmic network in the liver of mammals, then **Ranvier** (1885) and **Boehm** and **Davidoff** (1898) made identical observations.

In 1899, **Renaut** observed that, from the periphery of the nucleus,

"*delicate protoplasmic trabeculae radiate, intercepting by their contours meshes at the nodal points of which are the protein granules. These radiating meshes will join at the periphery of each cell a thin border formed by their fusion with each other over the entire periphery of the element, etc.*"

This observation by **Renaut** was confirmed in 1906 in a study by **Gilbert** and **Jomier** (24) on the liver cells of the dog and the rabbit. These authors noted that:

"*their protoplasm is composed of an areolar network whose meshes extend regularly over the entire surface of the cell. The nodal points are occupied by small granules with clear contours, presenting the same coloring affinities as the rest of the filamentous substance.*"

Let us note here that the granules grouped at the nodes of the protoplasmic network, that is to say at the intersection of the filaments constituting this network are, as will be demonstrated subsequently, the balls of the dumbbells constituting the cytoplasmic

network; these balls are assembled at the nodes of the network because the dumbbells are always articulated together exclusively by their balls, never by their rods.

Spongioplasma. In 1910, **Policard** (60) distinguished in the hepatic cell a protoplasmic framework, the spongioplasma, whose meshes are filled with a clear liquid, the hyaloplasma, more or less charged with glycogen. This spongioplasma was the network already described by **Küpfer, Klein, Ranvier, Bochm** and **Davidoff**, then **Renaut** and **Gilbert** and **Jomier**; but **Policard** noticed in the trabeculae of the spongioplasma a device of absolutely clear chondriosomes, in the form of short bacilli shaped chondrioconts. In each trabeculae, the elements are arranged one after the other, in a row. Some are applied tangentially to the nucleus or to the internal face of the membrane. The entire chondriome reproduces the design of the spongioplasma.

Renaut's observations in 1899, confirmed by those of **Gilbert** and **Jomier** in 1916, then those of **Policard** in 1910, the latter noting that "*the entire chondrioma reproduces the design of the spongioplasma*," should have quickly led to a complete understanding of the anatomical constitution of the cell.

But progress on this question did not continue, and this must be attributed to new studies on mitochondria, which from that moment onward captured the attention of bio-

logists to a much greater extent. These studies, moreover, had a very clear effect on the progress of knowledge relating to cellular organization because certain biologists attributed to the chondriome certain properties that it did not possess, which had the effect of presenting it from that moment onward as a system distinctly different from the cytoplasmic network.

The one who asserted himself most strongly in this direction was **Guilliermond** (27, 28, and 29); his observations constitute a denial of the existence of any cellular organization in both animal and plant cells. We will return to this point later in more detail.

Studies carried out since 1908 on mitochondria have led to these bodies being considered as autonomous, independent elements, without any relationship between them, and to them being attributed properties that they do not possess; the effect of these errors has been to make it impossible to study their principal function: that of being the constructive organelles of the cell.

***Ergatoplasma*. Solger** described in 1894-96 under the name of *basal filaments* a group of filaments generally located in certain places in the cell and stainable by nuclear chromatin dyes; they were later studied and called *ergatoplasm* by **Garnier** and the **Bouin** brothers (1897). Most authors who have studied ergatoplasm, notably **Prenant** (65), considered it a special aspect that the chondriome takes on with certain techniques. **Regaud** expressed a contrary opinion and considers, with **J. Mavas** (66), ergatoplasm and mitochondria as different and distinct formations.

It now appears clearly that ergatoplasm is the aspect of a cytoplasmic network more altered by the fixative than the formation that **Policard** described as spongioplasm.

Policard's conclusion that "*the entire chondriome reproduces the pattern of the spongioplasm*" provides us with further proof of the identity of all the protoplasmic formations that have been described since **Küpfer** (1875) under the names of protoplasmic or cytoplasmic network, ergatoplasm, spongioplasm, neurokeratin network, Lanterman network, pericellular network, Golgi network **(1)**. All these formations are *the network formed by the articulation exclusively by their balls of a group of elements of the elementary organelle, the dumbbell, to organize living matter.* Another form of this mode of articulation is the filament made up of dumbbells placed end to end. We will study it in other places.

(1) *Do not confuse this pericellular network with the intracellular formation generally referred to as the "Golgi apparatus."*

***Golgi apparatus*. Golgi** reported (9) in nerve cells of various types, the existence of a network called the **Golgi** reticular apparatus. According to **Golgi's** first observations, this network located near the nucleus would have no relation with it, nor with the external membrane and would be composed of fibrils or canaliculi grouped in a regular network presenting nodal swellings.

Some histologists have identified it with **Holmgren's** trophospongium, others with spongioplasma, others with chondrioma.

Ramon y Cajal (28 bis, p. 549), who studied this network, calls it **Golgi-Holmgren** *intraprotoplasmic canaliculi*, thus identifying the formations described by these two authors, which he considers to be a vacuolar or tubular apparatus. **Guilliermond** (11) concluded that this apparatus does not exist in animal cells and that it has no precise significance, because it appears, according to him, to relate to disparate elements belonging either to the vacuolar system or to other paraplasmic formations. As for plant cells, **Guilliermond** denies the existence of any formation of this kind.

The study of the **Golgi** apparatus is made, on the other hand, on page 96 regarding the search for the existence of the cytoplasmic network in neurons; it will be demonstrated there that, in nerve cells, this apparatus can have nothing in common with the cytoplasmic network, because the latter is a completely different formation; it is the pericellular network also discovered by **Golgi**, and whose seat is extracellular while the **Golgi** apparatus, or intraprotoplasmic canaliculi, is intracellular.

* * *

Following **Golgi, Holmgren** described (34) in the cells of the spinal ganglia, the liver, etc., a system of channels that he called "*Trophospongium*" that would connect the cytoplasmic space to the external lymphatic spaces, a connection he subsequently abandoned.

The studies in this volume demonstrate that these channels do not exist in the liver or in the cells of the spinal ganglia (see pp. 76 to 78 and plates 16 to 21). These studies demonstrate that the channels indicated by **Holmgren** in his drawings are artifacts due to the techniques he used.

Moreover, it is difficult to discuss this subject, so imprecise are **Holmgren's** drawings and so varied is the representation of these channels. A fair and reasoned assessment would only be possible by examining clear photographs of clear preparations.

Finally, the argument that dominates the entire question: these channels surely do not exist and have no reason to exist in the cytoplasm, because the latter is constituted, as demonstrated later, by a complicated network of dumbbell organelles articulated by

their balls, and the entire interior space between these dumbbells forms only a single cavity occupied by the hyaloplasm, a liquid medium that can circulate freely. Channels would only be useful if the hyaloplasm were gelatinous and non-mobile, which it is not.

For further details, I refer the reader to the study of spinal ganglion cells on page 76, and to the study of the constitution of the neuron, page 95.

* * *

***Vacuome, Spherome and Plastidome*. P.-A. Dangeard** has put forward a very special concept of the constitution of plant cytoplasm. Here, according to **Guilliermond** (27), is a summary of this concept:

The vacuoles of all the most diverse plants always contain, in a colloidal solution state, in their juice a substance that has an elective power for the vital colorants with which it precipitates in the form of corpuscles. Now, in the cells of the meristems, the vacuoles are found in a highly condensed solution state and take on the forms of small grains assembled in chains and filaments characteristic of chondriosomes. These elements, which Dangeard designates under the name Metachromes, swell by absorbing water during cell differentiation, then, by fusing, constitute large vacuoles with very dilute contents in which the vital colorants determine the precipitation of the colloidal contents...

***Dangeard**, through his vital observations, also noted the presence in the cells of the most diverse plants of small, highly refractile grains, which move rapidly in the cytoplasm and reduce osmic acid, to which he gave the name Microsomes or spherosomes...*

*For **Dangeard**, the chondriome is broken down into:*

***1. Vacuome** or **vacuolar apparatus**, certain initial phases of which (metachromes) offer mitochondrial aspects;*

***2. Spherome** or set of **microsomes** or **spherosomes**;*

***3. Plastidome** or set of plastids, the young forms of which (mitoplasts or spheroplasts) offer the characteristics of chondriosomes.*

As will be seen after reading the documents contained in this book, the concept of a vacuolar apparatus does not accord with the facts. The vacuolar cavities that **Dangeard** speaks of are abnormal cavities corresponding to a zone of destruction of the cytoplasmic organization. In his system, **Dangeard** does not mention or take any account of the cytoplasmic network, which he did not see.

* * *

In the preceding account, very little mention is made of the chondriome. This results from the fact that, in general, observers have considered network-like cytoplasmic formations and the chondriome as totally different.

Policard appears to be the first to have pointed out that, in the sections of the formation he called *Spongioplasma*, the elements of the chondriome, in the form of short chondriocontes, are arranged in rows and that the entire chondriome reproduces the design of the spongioplasma. But he did not see that the spongioplasma and the chondriome constitute only one and the same object.

The first observations of the presence of granulations in plant cells date from the beginning of the last century and are due to the work of **Mitscherlich, Schwann, Cagniard-Latour, Trécul**, and **Turpin;** these observations are of considerable importance because they are what made us aware that the brewer's yeast globules that generate the ferment are of plant origin and that "*we see them emit, by a sort of explosion analogous to that known in pollen vesicles, a prodigious number of very small colorless globulins which spread into space and whose swarming movement is very lively*" (36).

The conclusions of this work were never accepted by **Pasteur** (1), but they nonetheless definitively demonstrated the inaccuracy of the theory of atmospheric heterogeneity or panspermia and of the dogma of the aseptic nature of living organisms.

In current biological circles, **Altmann (1)** is still credited with the first knowledge of mitochondria. In bibliographical accounts of the chondriome, it is generally forgotten to mention that **Béchamp** (4), who was an illustrious French chemist and biologist, professor at Montpellier, then at Lille, reported, well before **Altmann**, the presence in the cellular elements of animals and plants of fine protoplasmic granules that he called microzymas, to which he devoted numerous studies over many years.

From his studies carried out from 1860 to 1890, that is, well before **Altmann**, he concluded mainly:

1. That it is these granules that are the agents of the chemical transformations of which the living cell is the seat;

2. That, when a cell dies, these granules or microzymas transform into bacteria and are the origin of the bacterial cultures that invade the tissues of animals and plants and of

(1) *Read on this subject the very interesting work of Professor Rappin, Director of the Pasteur Institute in Nantes on the Etiology of infectious diseases, p.97. (Brittany Printing House, 32, rue de la Fosse, Nantes, 1933.)*

the putrefaction they undergo under specific conditions, for example, in frozen tissues that have not been subject to any trauma.

The experiments he carried out on this subject, particularly on certain plants with a tough and very thick epidermis (4), as well as those he had carried out at Servel (75), are, in my opinion, impeccable and unassailable.

The facts observed by **Béchamp** were never accepted by **Pasteur**, despite the interventions of **Pouchet, Claude Bernard, Frémy**, etc., and healthy plant and animal organisms are still considered in principle as aseptic by his school. These facts were the subject of endless controversies between **Pasteur** and **Béchamp** which, combined with the injustices of scientific opinion towards the latter, saddened the existence of this biologist who will remain one of the most illustrious of those who have honored our country. The complete account of **Béchamp's** research and his controversies with **Pasteur** can be found in his work: *Conferences*, etc. (4).

It is therefore well established by these quotations that the presence of protoplasmic granules had been noted and their properties studied, notably by **Béchamp**, who called them microzymas, well before **Altmann's** publications on his granules.

But it must be recognized that **Béchamp** did not provide any information on these granules that sheds light on their forms, their location, their characteristics, their function, etc., in the cellular elements.

It was **Altmann** (1) who, in 1894, was the first to describe granules or bioblasts in animal tissue cells, to which he recognized two forms: a granular form and a filamentous form, the latter comprising either intricate filaments forming a network, or bacillus - shaped rods arranged in rows. For **Altmann**, each granula has an independent life and represents the most elementary living organic particle. Granules or bioblasts play the active role in the cell, carry out chemical transformations, and can themselves transform into filaments. For **Altmann**, each cell would be a colony of bioblasts.

In 1908, **Arnold** described his *plasmosomes*, which are nothing other than **Altmann's** granules, and to which he attributed the same role in the chemical transformations of the cell.

In 1902, **Benda** studied these same bodies, which he called *mitochondria*, and which were already well known before him, even in their filamentary and networked forms. He observed filaments containing granules arranged in chains, which he called chondriomites.

Finally, **Mèves** (1907-1908), in new observations that, like those of his predecessors, established the reality of **Altmann's** granules and filaments, called the rod-shaped mitochondria *chondriocontes*.

Altmann observed his granula by fixing the tissues with a mixture of equal parts 5% potassium dichromate and 2% chromic acid. He stained hot with a solution of acid fuchsin in saturated aniline water. He then differentiated in a saturated solution of picric acid in absolute alcohol diluted with 1/3 distilled water.

Arnold fixed in **Flemming's** solution and stained with a mixture of malachite green and corn yellow, then differentiated with 1% acidulated alcohol.

Benda used **Flemming's** solution as a fixative, containing only 2 or 3 drops of acetic acid per 20 cc, then stained with krystal violet and sodium sulfalizarinate.

Thus, after **Mèves'** observations, the existence of a group of elements called chondriosomes in the living cell was known, which included:

1. Round granules-**Béchamp's** microzymas, **Altmann's** granulas-or **Benda's** mitochondria;

2. Rods or filaments called chondrioconts by **Mèves** and already seen by observers who preceded him.

From this time and later, the work applied to a systematic study of mitochondria and the chondriome is so numerous that it is impossible for me to provide a complete analysis here.

Some of them have been applied to the study of so-called mitochondrial techniques, which, it is claimed, respect the integrity of the cell and especially its chondriome. We will examine their value later.

These studies, in which many researchers have specialized, are now part of a special branch of microscopic anatomy: Cytology.

Among the cytologists who have most thoroughly studied mitochondria and cytoplasmic organization over the past twenty years and have specialized in them, **Guilliermond** must be mentioned, whose results are authoritative, judging, for example, by **Nageotte's** assessment of them in his book: *The Organization of Matter,* which we will discuss later.

It is because of the notoriety acquired by **Guilliermond** in scientific circles on the question of the chondriome and the organization of the cytoplasm that I considered him the most qualified to teach me, through the study of his publications, the technique of researching mitochondria and their properties, which I was criticized for not knowing because my conclusions were opposed to currently and generally accepted notions.

We will take here, as a subject of study, one of his main memoirs entitled: *New research on the normal morphological constituents of the cytoplasm* (Revue d'analyse microscopic, t. XX, 1924, p. 1). We will begin by studying the techniques used. For each point studied, I will put in front of the controlled results the results of my personal observations.

II. STUDY OF THE CYTOLOGICAL RESEARCH TECHNIQUE

Let us first outline the technique used by **Guilliermond**. He used only so-called mitochondrial fixation methods; of the fifteen plates accompanying his dissertation, he used **Regaud's** fixative for thirteen of them and the **Mèves, Kull**, and **Benda** fixatives for the other two.

Guilliermond thus appreciates **Regaud's** method (27, p. 6):

*All of our research has shown us that among the mitochondrial techniques, **Regaud's** method (fixation with a mixture of formalin and potassium dichromate) is the only one that gives reliable results in plant cells. It can therefore be admitted without exaggeration that a result will only be certain if it has been obtained by this method.*

Let us note immediately that this statement is demonstrated to be completely inaccurate by all the drawings in his memoir showing cells fixed by this method; the reader will find further on the formal, indisputable proof of the total destruction of the cytoplasmic organization in these cells. On page 7, **Guilliermond** adds:

Our previous research has demonstrated that mitochondrial techniques preserve the chondriome as well as possible in the form it presented in the living organism. We have meticulously compared the appearance of living cells from the epidermis of Tulip and Iris, as well as of fungi belonging to the Saprolegnaceae, which are very favorable for vital observations, with sections fixed and stained by mitochondrial methods; we have been able to observe that these methods achieve the most perfect fixation possible of the chondriome. The same comparisons were made in the research we are going to present on the cells of Elodea Canadensis. Mitochondrial techniques are therefore beyond all criticism, provided that we take into account the reservations we have made regarding the use in certain cases of fixatives containing osmic acid.

Our observations obtained by mitochondrial techniques have always been verified as often as possible by vital studies, and we have never formulated the slightest interpretation that has not been subjected to this verification. We consider that in cytology, the only certain facts are those that have been verified in living organisms. Wherever vital control is impossible, only hypotheses are permitted.

These conclusions, including the one concerning the alleged necessity of vital control, are as inaccurate as the previous one. We will prove it.

It will be demonstrated by multiple proofs in this work that the cytoplasmic organization of the cell is achieved by a network of elementary organelles of a single type in the shape of dumbbells articulated exclusively by their balls, either among themselves, or with the external cell membrane, or with the nuclear membrane, and circumscribing multiple polygonal meshes in which the cytoplasmic liquid circulates freely and at all points. The dumbbells included in this network are permanently immobilized.

Apart from them, there are no other elements represented in the cytoplasm, apart from very fine granules animated by Brownian motion, which will be discussed about the bacterial function of living beings.

However, **Guilliermond** and others before and after him noted the presence in the cytoplasm of free, scattered elements presenting multiple, extremely varied, incoherent forms, which are currently considered to be mitochondria, that is, the chondriome of the cell.

It will be demonstrated later, by multiple proofs, that these free, scattered, and shapeless elements contained in the cells drawn by **Guilliermond** in his plates are the scattered debris of the disorganized cytoplasmic dumbbell network, dislocated by the altering action of the fixative and whose elements are already largely destroyed.

The origin of the nature of these free and scattered elements, the cause of their very diverse forms, cannot be contested with the slightest chance of success, because I have demonstrated the origin of each of these forms, to which portion of the dumbbell organelles it corresponds, and its place in the cytoplasmic network; this demonstration has been given in a manner as precise and as incontestable for the amyloplasts.

Here it is demonstrated that this mitochondrial fixative, which so perfectly preserves the integrity of the cytoplasm, completely dislocates and destroys it.

* * *

Let us now examine the technique of vital observation. According to **Guilliermond** (27, p. 7):

In cytology, the only certain facts are those that have been verified on living organisms. Wherever vital control is impossible, only hypotheses are permitted.

But on the following page, **Guilliermond** adds:

Since vital observation almost always gives figures that are less clear than those obtained by staining fixed sections, it is always preferable to approach it only after the method of fixed and stained sections. In this way, it is much easier to understand the less clear images provided by vital observation.

This new remark seems to impose on the contrary the conclusion that vital control is insufficient and that hypotheses are the only ones permitted wherever the method of observing correctly fixed and stained sections is impossible.

The confrontation of **Guilliermond's** cell drawings with the photographs of similar cells that I exhibit in plates 1 to 15 demonstrates, beyond the slightest doubt, that in the former, the organization, that is to say the cytoplasmic network, was completely destroyed.

Under these conditions, since **Guilliermond** claims that *he never formulated the slightest interpretation that was not subject to vital verification*, it must therefore be concluded that, in living cells, that is, cells necessarily having an intact cytoplasm, he would have observed both the existence of enormous vacuoles, then the absence of the cytoplasmic network and finally the presence of the shapeless, scattered, distinct and completely unrelated elements that constitute the irrefutable, incontestable proof of the destruction of the cytoplasmic organization and its elements.

Now, it is impossible to be able to observe such destruction in a truly living and intact cell, because it always possesses a cytoplasmic network articulated on the one hand to the external cell membrane, on the other hand to the nuclear membrane, a network that maintains the fixity of the position of the nucleus at the center of the cell.

Guilliermond could not have observed the existence of the normal cytoplasmic network in a living, intact cell. We can be certain that, if the cytoplasmic network were clearly visible and, above all, easily stainable with neutral red, many observers would have seen and photographed it long ago, for example **Lewitsky** (41), whose photographs of living cells show no trace of this network; the clear photographs of living cells from *Elodea Canadensis* leaves that I took and which are contained in plate 12 do not show it either. **Guilliermond's** opinion on this point has undoubtedly evolved since he writes on page 42 of his treatise on cytology:

Since vital observations are not sufficient for the study of cellular structure, the various constituents of the cell having too small a difference in refractive power to be very distinct and the vital dyes being of too limited use, it has been necessary to resort to the so-called fixation method.

Let us now move on to the examination of formalin fixation. On page 6 of his memoir, **Guilliermond** assesses it thus:

Some authors are satisfied with simple formalin fixation. The process is sometimes successful, but very often formalin also gives very defective results, probably due to its acidity, and it is certain that the action of potassium dichromate plays a very important role. Formalin fixation is therefore not recommended.

However, we read on page 1015 of **Langeron's** *Précis de Microscopie*:

Formalin alone preserves mitochondria remarkably well, perhaps better than chromic salts. On the other hand, it does not insolubilize fats unless the action of formalin, alone or mixed with potassium dichromate, is followed by a complementary fixation or postchromatization.

I have never found that adding potassium dichromate to formalin improves fixation; I have obtained poorer results with this mixture than with formalin alone.

As for postchromization, I have not observed that it improves fixation. **Guilliermond** had already made this observation. **Parat** (56), who considerably shortened the duration of this operation by performing it in an incubator at 38°C for 48 hours or more, certainly did not succeed in obtaining a better result because the cell drawings he published there attest to the total destruction of the cytoplasmic organization. I have reproduced one of these drawings (23, pl. 2) representing the cells of the pancreas of Axolotl in figure 2 of plate 21 of this volume. **Parat** had used **Regaud's** mixture as a fixative. It is obvious that postchromization, whatever the process used to perform it, is incapable of restoring an organization that has already been destroyed by the fixative mixture. We also read in **Langoron's** *Précis de Microbiologie*, on page 1014:

The only fixative agents that preserve almost the entire contents of the cytoplasm are osmic acid, chromic acid, potassium dichromate, and formalin.

But on page 338 of this same work, **Langeron** wrote: *Chromic acid fixes the nucleus very well, but profoundly alters the cytoplasm.*

I have found that all mixtures of chromic acid and osmic acid, even those without added acetic acid, destroy the cytoplasmic network, even if they contain only a minimal proportion of the latter, such as **Benda's** mixture, which is **Flemming's** mixture with a very low acetic acid content.

I have carried out numerous comparative tests with these various fixatives on the same material, all the other operations of the technique being perfectly identical up to the final examination. It is with formalin used alone that I have obtained the best or least defective results, because, like all the others, this fixative alters the cytoplasm more or less profoundly.

These alterations have been attributed to the formic acid it contains; it is easy to avoid this objection by neutralizing it, but it appears unfounded, because I have obtained results that are just as good, and have even sometimes appeared a little more favorable, with formalin solutions containing from 0 cc. 25 to 0 cc. 50% acetic acid.

The harmful action of formalin solutions on the cytoplasm may result in part from the fact that they do not contain the various salts contained in the cytoplasmic network and liquid. To counter at least part of this possible cause of alteration, I added to the perfectly neutral formalin solutions all the constituents of **Locke's** liquid or **Ringer's** liquid, as the case may be. These solutions seemed to me to give results that were a little more favorable than solutions of formalin alone.

* * *

Lison strongly criticized the use of formalin as a fixative in his treatise on *Animal Histochemistry* (**Gauthier-Villars** 1936) without, however, justifying this criticism with any specific facts. We read on page 10 of this treatise:

Many histologists, when they want to carry out histochemical research, believe they are doing the right thing by using so-called "indifferent" fixatives (?). Most often, they choose absolute alcohol or formalin. They are afraid of using fixatives recognized as the best in histological technique. This is a mistake.

Further on, on page 12, we read:

We must strongly protest against the preference often given to "indifferent" fixatives, which are not, moreover, as indifferent as is said.

It would be useful to know the reasons that can support such an assertion, which the facts presented previously demonstrate to be justified.

If many authors choose formalin as a fixative, it is probably because it seems to them to better achieve than other fixatives one of the essential conditions of all histochemical research, which is, as **Lison** indicates, the preservation of the structural integrity of the tissue studied.

And this choice seems excellent if we believe **Langeron** who, in his *Précis de Microscopie*, wrote on page 1015: *Formalin alone preserves mitochondria remarkably well, perhaps better than chromic salts.*

And on page 335: ***R. Noël*** *and* ***G. Mangenot*** *recognized that formalin, which is both neutral and salty (8% formalin in the physiological solution), fixes the nucleus well and does not chemically modify the cellular contents.*

If we consider, on the other hand, that oxidizing fixatives, osmic and chromic acids, dichromates, etc., have a strong chemical action and that they modify the chemical constitution of intracellular bodies, it is quite natural that many observers do not want to use them for their histochemical research.

If we also consider that formalin is not an indifferent body, but a relatively inactive reducing body, we can conclude that the choice of this fixative is perfectly justified, contrary to **Lison's** opinion.

Finally, how does he recognize the fixatives reputed to be the best in histological technique? No doubt from the assertion by certain authors of the superiority of the results they have obtained.

It is often difficult to judge the value of such claims. For example, chromic acid is indicated in **Langeron's** excellent *Précis de Microscopie* on page 1014 as "*preserving roughly the contents of the cytoplasm*," and on page 338, as profoundly altering it. The same is true for formalin, which, despite the two quotations just made, is indicated in this same *Précis* (p. 335) as a poor histological fixative.

But, even when the near unanimity of observers attests to the perfection of a fixative, it is imprudent to rely on this attestation without checking it, as proven by the example provided in this volume regarding mitochondrial fixatives, which have been attributed the quality of rigorously preserving the cytoplasm in the form it possesses in the living cell and of being beyond all criticism.

Indeed, the documents published by the observers who most categorically affirmed this preservation incontestably prove that the cellular elements they had treated with one of the most widely used mitochondrial fixatives (**Regaud**) had their cytoplasmic organization completely destroyed (cytoplasmic network) and no longer presented any relationship with the normal and real structure.

In reality, their results and the assertion they had formulated meant only that mitochondrial fixatives *preserve the chondriome in the same state as their predecessors wrote*, and these had, in general, described only the profoundly altered elements of a disorganized cytoplasmic network broken up into scattered fragments that were called mitochondria.

This is how false notions are transmitted over a long series of years, due to the absence or insufficient control of the techniques and results that were their starting point.

If I was able to provide proof of such errors, it is because, through a long and very meticulous study, I determined the action of fixatives on the material studied, the optimal proportions that should be given to the substances they contain, and the optimal duration of their action.

Lison's treatise on *Animal Histochemistry* provides us with another example that shows the difficulty of assessing the real value of a fixative. On page 11 of this treatise, we read about the fixation of chromosomes:

*It is after fixation with **Champy** or **Flemming-Heitz** (**Flemming** without acetic acid), which are among the best chromatin fixatives, that their structure is best preserved and best withstands the manipulations of the reaction (**Feulgen** nuclear reaction).*

This is an assessment that the facts demonstrate to be inaccurate. For example, the chromosome fixatives used by **Grégoire** and his students did not allow them to see that in the euchromocentric nuclear type, the euchromocenters are not isolated chromatic granules, but only the external balls of the radiating nuclear dumbbells that constitute the nuclear organization. They were unable to see this nuclear structure because the rods of the dumbbells were destroyed by the fixatives, and among these, the chromo-osmic fixatives in particular.

Now, we will be able to see in this book that I was able to demonstrate this nuclear organization using formalin, which **Lison** considers a poor fixative.

Another example is even more significant. If some cytologists were able to see homogeneous nuclei, that is, nuclei possessing neither figured elements nor structure, as for example in certain species of *Cucurbitaceae*, it is because these figured elements were destroyed by the fixatives used and, in this case, *the chromatin itself was destroyed.* The accuracy of this conclusion is established by the fact that other observers have succeeded in demonstrating the existence of figured elements in nuclei known to be devoid of them. I myself have demonstrated in this volume that the nuclei of *Elodea Canadensis*, which had been reported as homogeneous, possess a very complete structure of radiating dumbbells connecting the nucleolus to the nuclear wall.

This work proves that the organization of the cytoplasm and the nucleus is constituted exclusively by dumbbell organelles united by their balls, either to the cellular or nuclear membranes and the nucleolus, or to each other in a network containing in its meshes the cytoplasmic fluid or the nuclear fluid. This structure is constant and is never lacking in any animal or plant cell nucleus. The presence of a nucleolus in particular implies the existence of the nuclear dumbbell network.

* * *

From the numerous tests I have carried out with the most diverse fixatives, I have concluded that all of them alter to a greater or lesser extent the elements that constitute cellular organization; this alteration exists even with the most successful fixation.

What is important to seek, therefore, is a minimum of alteration, and it is only by determining the optimal proportions of substance in the fixative and the optimal duration of its action on the material studied that this can be achieved.

By carrying out this research on several fixatives, we can, by choosing the most favourable one, obtain sufficient preservation of the cellular structure for the study.

Despite these preliminary determinations, the results of the fixations are irregular, without the cause being perceived; this is a known fact. Also, during the research and despite the preliminary determinations, I have often carried out fixations simultaneously on similar fragments of the same material, either with different concentrations of the fixative, or with variable contact times, 1, 2, or 3 days, etc., sometimes much less.

Fixation was generally carried out either with a 10% formalin solution alone, rarely with a lower proportion (6 to 10%), or with a formalin solution to which the constituents of **Ringer's** liquid were added. Very often, fixations were carried out simultaneously and comparatively on the same material with bichromated formalin (**Regaud**). But in general, these preparations were not used due to the defect in the results.

The fixed fragment, then washed, was dehydrated in absolute alcohol, then treated with toluene and finally embedded in paraffin in an oven at approximately 56° (Péchelbronn paraffin fusible at 54°).

Staining was most often done on sections with ferric hematoxylin, either alone or followed by cold staining with **Zichl's** fuchsin with the addition of 1/3, 1/2, or 2/3 95% alcohol, or with acid fuchsin, or erythrosine. In general, staining was done with ferric hematoxylin alone using the following procedures:

The deparaffinized sections were mordanted for 1 to 10 minutes in a 0.50 or 1% aqueous solution of iron alum at a temperature of 40°C, washed quickly in distilled water, then stained at a temperature of 40°C for 10 minutes in a 1% hematoxylin solution, at least one month old and obtained by mixing 80 cc of distilled water, 10 cc of 950 alcohol, 10 cc of glycerin, and 1 g of hematoxylin; finally, they were washed quickly in distilled water and then immersed for 2 or 3 seconds, or a little longer, in a 0.25% lithium carbonate solution to transform the black color into a dark blue color, and finally washed in distilled water.

In some cases, when it was desired to obtain a progressive coloration monitored and stopped at the desired point, without differentiation, mordanting and staining were carried out in the same solutions, but cold for the necessary time, mordanting being carried out for 30 to 60 minutes.

Microscopic examination of the sections was generally carried out without differentiation, but when this was deemed necessary, it was done either with a saturated solution of iodine in alcohol, or with a 0.25% lithium carbonate solution, or with a 1% ammoniacal iron alum solution, or with alcohol saturated with picric acid.

In the vast majority of cases, the preparations were studied without having undergone any differentiation, the staining with ferric hematoxylin having the main aim of making all the figured elements visible and very apparent, and the differentiation having aimed, for the study of the cytoplasm, only to make the points that were too strongly colored more permeable to light.

A very important point that I consider part of the research technique is the photography of the preparations studied. All the interesting details observed during the study of a preparation are photographed on a 9 x 12 plate. Each photograph is taken in 5 to 10 minutes at the most. The plates, set aside, are developed in groups of 6 or 12 for 20 minutes in a Glyconyol solution containing 25 cc of the following concentrated solution for every 220 cc of distilled water, which must be stored in full, well-stoppered bottles:

Anhydrous sodium sulfate……..80
Anhydrous sodium carbonate….100
Potassium carbonate……150
Glyconyol……..50
Distilled water……..1,000

The quickly washed plates are fixed in sodium hyposulfite for 15 minutes and finally washed for at least 2 hours in running water. The plates that have given me the best results are the SE plate and the Lumière micropanchro plate. From each shot, two paper proofs are made, one normal, the other a little more fixed, in order to better bring out the light details.

The Lumière micropanchro plates give very satisfactory results, but the sensitive gelatin layer has the serious disadvantage of being perforated with multiple holes due, it seems, to glass dust remaining adherent to the surface before the application of the sensitive layer and this defect has not been removed although it has been reported; these holes give more or less large black spots on the prints. It is highly recommended, to avoid other imperfections in the prints, to pass a cotton ball, then the palm of the very dry hand on the gelatin of the blank plate to remove the dust before including it in the frame.

We thus obtain a series of photographic prints containing, for each preparation, the perfectly exact recording of all the interesting images observed, because the lens and the photographic plate make no mistakes, forget nothing, and distort nothing.

The study of these images almost always makes it possible to observe arrangements that the eye had not noticed upon direct examination, to complete an incomplete observation, to clarify poorly seen details, and often also to rectify an error.

These preserved documents often take on inestimable value much later, the observation of a new fact, after many years, requiring a retrospective check of old observations which, very often, is no longer possible due to the alteration of the original preparations.

This process of regular photographic recording of all observations makes it possible to obtain much more important and much more reliable results than the method of annotation and small drawings; it eliminates the intervention of memory which, in addition to forgetting, can lead to error.

But, apart from these advantages, the photographic recording of the image that one wishes to exhibit is rigorously faithful, not subject to error, and represents the absolute truth that the reader can himself reestablish in the event that the author has made an error of interpretation.

On the other hand, drawing, which first has the disadvantage of often requiring long hours instead of the 5 to 10 minutes required to execute a photograph, has, in addition, the serious disadvantage that can be described as prohibitive, of being necessarily inaccurate for the following reasons:

1. All the images of microscopic preparations are so complicated that an exact, complete drawing is unrealizable; photography alone produces a rigorously faithful image;

2. The observer who draws, represents what he sees and what he believes he sees, but *he cannot represent the details that he has not noticed and that have escaped him.* The lens and the photographic plate forget nothing, distort nothing. The observer who draws represents objects *as he understands them and not as they are, and he introduces his errors of interpretation into his drawing*.

The photographic recording of a fact, if it is carried out properly and according to all the rules, can therefore alone ensure the complete guarantee of its accuracy and it gives an incomparably greater value than a drawing to the author's conclusions.

Finally, the reader can follow the presentation of a question as well and even better than if he had his eye under a microscope, when well-made and sufficiently numerous photographic images are placed before his eyes; this is a considerable advantage for the clarity and precision of the demonstrations.

Microscopic photography, an absolutely perfect recording method, presents such a immense advantages from every point of view and is likely to accelerate the progress of science so greatly, that it is difficult to understand why it is not used exclusively in biological research, drawings being reserved for schematic conceptions and excluded from observation. Photographic recording is so simple, so easy, so rapid, that it is unacceptable not to use it; the failure to use this recording method suggests that the author has observed defective, unpresentable preparations, subject to dubious interpretations.

III. STUDY OF THE CHONDRIOME AND THE STRUCTURE OF THE CYTOPLASM OF PLANT CELLS

At the beginning of this study on the organization of the cytoplasm and to facilitate the understanding of the critical studies that follow, I believe it is necessary to first demonstrate the indisputable existence of the cytoplasmic network, its constitution by the universal dumbbell organelle, and the shape and mode of assembly of this organelle. We will study successively:

1. The structure of the cytoplasmic organization of plant cells.
A. The structure of the cytoplasmic organization in adult cells.
B. The structure of the cytoplasm of young meristem cells.
C. The modifications of the cytoplasmic organization during the evolution of young meristem cells to the adult state.

2. The altering action of fixatives on the cytoplasm and the formation of vacuoles.

3. The state of destruction of the cytoplasmic organization in the cells that were the subject of **Guilliermond's** research on the normal constituent elements of the cytoplasm of plant cells.

4. The nature and origin of the scattered elements, unrelated to each other and so varied in form, that appeared in these cells. The cause of each form of element and their place in the cytoplasmic network.

1. Structure of the cytoplasmic organization in plant cells.

A) STRUCTURE OF THE CYTOPLASMIC ORGANIZATION IN ADULT CELLS

I will show this network, firstly, in the adult cells of the radicle of the *Faba vulgaris* bean where I was able to observe it in a good state of preservation. Beans are germinated on moist filter paper in a crystallizing dish and the radicle is removed when it reaches a length of 10 to 12 millimeters. It is divided longitudinally into 2 equal parts and the pieces are fixed for 3 days in 12% formalin **Ringer's** solution. Sections of 5 microns and 2 1/2 microns are made.

The deparaffinized sections were stained with ferric hematoxylin and immersed for 2 or 3 seconds in a 0.25% lithium carbonate solution only to turn their color blue; they are refrained from differentiating them.

In these preparations, the cells show a very beautiful cytoplasmic network, like those photographed in plate I. Despite the beauty of these networks, they are not intact; they are already altered by the fixative and this alteration, which should not be regretted here, will on the contrary serve to establish that the vacuoles, such as those that **Guillermond** has depicted in his drawings, are indeed zones of destruction of the cytoplasmic network and not elements of the cytoplasmic organization.

Figure 1 of this plate first shows cells (magnification 335) whose network is almost intact and others where it is more or less altered. In some of these, we notice large holes which are vacuoles at the level of which the cytoplasmic network is destroyed. Figures 4 and 5 are photographs at a magnification of 750 of two cells from the same region whose cytoplasmic network is quite strongly altered; the reader will nevertheless see under a magnifying glass the dumbbells constituting the network and their articulation by their balls.

Figures 2, 3, and 7 show cells from the same field not far from the terminal meristem, photographed with a different focus for Figures 2 and 3 (higher magnification (1100) for Figure 7). Figure 6 represents two cells from an immediately neighboring field (Gr. 1100). Figure 1, Plate 2 shows, enlarged (Gr. 2200), the cells from Figure 7, Plate I.

To clearly observe the variable shape and dimensions of the dumbbells as well as their mode of articulation by their balls, the reader will benefit from examining the figures in Plate I with a magnifying glass. He will thus be able to see the rigorous accuracy of the following conclusions:

1. That in the face of such photographs and such an obvious, clear, and understandable cellular organization, there can be no question of questioning the cytoplasmic network.

2. That this network is formed by organelles of a unique dumbbell-shaped type, consisting of a rod of variable length bearing a ball at each of its ends, and that these balls are almost always less chromatophilic than the rods.

3. That these dumbbells articulate with each other by their balls and thus form meshes of the cytoplasmic network and this network itself, which implies that the balls of the dumbbells are made of a very adhesive substance. This articulation of the dumbbells by their balls explains why some observers, such as **Renault-**1898, **Gilbert** and **Jomier-**1906, observed that fine protoplasmic granules occupy the nodal points of the fine aerolar network constituting the protoplasm.

4. That the rods of the dumbbells correspond very visibly, in the cell, to the mitochondrial elements called chondrioconts, while their balls are the granular chondriosome and that thus, it is demonstrated:

A. *That the elements called mitochondria are never free,* a chondriocont always bearing, normally, a ball or granular chondriosome at each of its ends, a combination which constitutes an elementary dumbbell organelle.

B. That even when joined together to form a dumbbell organelle, they are definitively immobilized in the cytoplasmic network constituted by the articulation of the dumbbells between them, and that they can only become free again by the dislocation and destruction of this network and of the dumbbell organelles themselves.

5. *That the fact of observing that the dumbbells or their debris are free and unconnected to each other in the cell is certain proof of the dislocation and disorganization of the cytoplasmic network and consequently of the destruction of the cell.*

6. *That, consequently, the figurative elements that have been called mitochondria,* that have been described as free, independent, and even mobile elements, *have no real existence, do not exist.* They are the scattered debris of the dumbbell organelles and the destroyed cytoplasmic network that have been described under this name.

Let us note here that all these conclusions receive new confirmations in most of the following plates.

B) STUDY OF THE STRUCTURE OF THE CYTOPLASM OF THE YOUNG MERISTEN CELL

Young meristem cells have a characteristic and constant structure. They have a nucleus of considerable volume compared to that of the cytoplasm (sometimes almost equal).

The distance between the periphery of the nucleus and the cell membrane does not exceed one-third or two-fifths, sometimes one-quarter of the diameter of the nucleus in very young cells. Under these conditions, the nucleus is held in place, in the middle of the space externally limited by the cell membrane, by multiple flying buttresses, the direction of which is exactly that of the radius of the nucleus which it extends.

These flying buttresses are dumbbells, one of whose balls is attached to the external wall, and the other, attached to the core, often appears to be included, either in whole or in part. The appearance of the young cell thus formed is therefore quite characteristic. In a section, it has the appearance of a car wheel with a very large hub seen from the front and whose spokes are the flying buttresses of the nucleus.

This radiating arrangement of dumbbells around the nucleus persists unchanged in adult cells with several rows of dumbbells. This constitution is therefore completely different from that represented by **Guilliermond** in Figure 3, Plate 20, and in Figure 1, Plate 1, of his memoir. Figures 3 and 4, Plate 3 of this book show it in several cells of two very young axillary buds of *Elodea Canadensis*. This typical constitution can also be seen at high magnification in the young cell located at the right corner of Figure 3, Plate 5 (magnification 1100) and in two cells of Figure 4, Plate 5, cells located, the first quite far from the terminal meristem of the stem, the other two at the base of a young leaf at the end of the stem.

Figure 1 of Plate 3 is the photograph of Figure 3 (p. 20) of **Guilliermond's** memoir; it is a drawing representing the cells of the terminal meristem of a leaf rudiment of an axillary bud of *Elodea Canadensis.* Examining the cells of this terminal meristem, we are led to the following observations:

1. They do not present the appearance that the meristem cells show in tissue sections, that of an automobile wheel with 10 to 15 spokes;

2. We do not see the slightest trace of cellular organization. That is to say, dumbbells which radiate around the nucleus and which are the flying buttresses which maintain it in the center of the cell. We see only a single club-like shape, scattered and isolated at the top right of the drawing;

3. In sections such as those in Figures 2, 3, and 4 of our Pl. 3, there are always a certain number of cells affected by the cut at the level of the external membrane, and whose polygonal mesh network that constitutes it can be seen. This network is visible in Figure 4, at the end of the meristem and on the right (cell in the 2nd row). This arrangement is completely absent in the figures in **Guilliermond's** drawing. Figure 2, Plate 3, shows this

same network in various cells located at the level of the insertion of a cotyledon pedun cle on the embryo of the germinating bean seed.

The polygonal mesh network can be seen, either inside the cell or outside, representing the external membrane. Several young cells have retained the single-row structure of dumbbells;

4. On the other hand, we see in the cells of this drawing scattered, independent elements, without any connection between them, long undulating filaments, short rods, grains, etc., which are the debris of the dumbbells of the cytoplasmic network.

We are thus led to conclude that, quite clearly, the organization of these terminal meristem cells examined and drawn by **Guilliermond** no longer existed, and this obviously because it had been destroyed.

This necessarily also leads to the conclusion that the scattered elements of various shapes, drawn by **Guilliermond**, are the remains of altered and dislocated dumbbells. Note that, in the meristem cells, there exist only the short rods of the dumbbells and no long flexuous filaments like those depicted by **Guilliermond**, filaments which he indicated, in the legend of figure 1 of plate 1 of his memoir, as representing the plastids; this erroneous indication, due to the fact that these filaments are only debris, is all the more surely so since it will be demonstrated later that the plastids do not exist.

C) CHANGES IN CYTOPLASMIC ORGANIZATION DURING THE EVOLUTION OF YOUNG MERISTEM CELLS TO ADULT AGE

If we examine a section of the end of the stem of *Elodea Canadensis*, we see that as the observer's eye moves away from the terminal meristem, the volume of the young cell

increases progressively, and this is mainly due to the enlargement of the cytoplasmic space, but the structure does not yet vary, and this up to a point quite far from the end of the stem; in the upper half of Figure 1, Plate 4, for example (magnification 50), the cytoplasmic structure has not yet changed appreciably in many cells, but as one moves away from the terminal meristem, the cells are larger, the cytoplasmic space is larger, and the rods of the dumbbells have become longer.

This can be seen more easily in Figure 5, Plate 4, which represents the photograph at magnification 100 of the same *Elodea* stem as that of Figure 1, but taken on a very similar section. The region photographed corresponds to the lower half of the stem in Figure 1 and it shows on the right the section of an axillary bud with leaf primordia and leaves. In this photograph, the persistence of the primitive type of organization of the terminal meristem cells and the axillary bud is clearly visible.

Moving further away from the tip, we notice more and more cells whose cytoplasmic space has increased further, and whose membrane is connected to the nucleus by two rows of articulated dumbbells, instead of just one, an arrangement which corresponds to the beginning of the formation of the cytoplasmic network proper. For example, at the upper edge of Figure 3 (magnification 200), some cells still have only one row of radiating cytoplasmic dumbbells while in the lower quarter, many have two; Figure 2 represents the lower half of Figure 3, at about 335 magnification. Finally, the further we move away from the terminal bud, the more the cytoplasmic space is developed and the more the cytoplasmic network increases.

Plate 5 provides new indications: In Figure 3 (1100 enlargement), we see, on the right, a cell that has retained the primitive type with a single row of radiating dumbbells. This cell is located in a region where cells with two rows of dumbbells are already found, such as the cells in Figs. 2, 5, and 7 (1100 enlargement). Fig. 1 (1100 enlargement) shows, on the left and above, a large cell located in an even more distant region and whose cytoplasmic network comprises 4 to 5 rows of meshes.

It should be noted that very often, in the young leaves of *Elodea Canadensis,* and especially in the region of their base, the cells retain the primitive type with a single row of radiating dumbbells; Figures 4 and 6 (1100 enlargement) show several of these cells. Finally, note that the large cell in Figure 7 of this plate (*Elodea Canadensis*) is the one that first showed me the morphological constitution of the nucleolus, which will be explained later.

* * *

This study has therefore demonstrated:

1. That in young meristem cells the cytoplasm has a particular organization comprising only a single row of radiating dumbbells, one ball of which is articulated to the cell membrane and the other, at the periphery of the nucleus, with the peripheral balls of the nuclear radiating dumbbells; there is therefore no true cytoplasmic network in young meristem cells;

2. It is only later, as they grow and develop in the centrifugal direction, that they acquire a second, then a third, fourth, etc., row of dumbbells, that is to say, that the cytoplasmic network develops;

3. The scattered elements, of varied and independent shapes, without any connection between them, grains, rods and filaments that **Guilliermond** described in the cells of the meristems do not exist there in the normal state; they are, at the very least, the grains and rods, the debris of the destroyed normal elements, the radiating dumbbells;

4. The meristem cells observed by **Guilliermond** had their cytoplasmic organization completely destroyed by the fixative or by another preparation technique. The author did not report or show the slightest trace of the cytoplasmic organization.

Let us point out, in closing, that **Guilliermond** believed he could deduce from his observations on the elements described by him in the cytoplasm of meristem cells, the existence of two lines of mitochondria, one inactive, the granules, the other active, the rods or chondrioconts which, in evolving, would transform into chloroplasts or amyloplasts.

Let us observe from now on the impossibility of the exactitude of such a distinction and of a such a faculty of evolution of chondrioconts, all these elements being only mortified debris destined to disappear and not to evolve. This question will be studied in more detail on page 48.

2. Alteration of the cytoplasmic network by fixatives and formation of vacuoles.

The study of the figures in plates 1 and 2 will, moreover, allow us to show that vacuoles are areas of destruction of the cytoplasmic network; in the largest cell in figure 1, plate 2, at the top and to the right of the nucleus, we see four vacuoles in the process of formation, but in which we can still faintly see the balls and rods of the barely colored dumbbells. We also see several in formation in figure 2, plate 2; in one to the left of the nucleus, and in the other to the right and a little below, we also see the elements in the process of destruction and, notably, in the latter, a strongly marked dumbbell, one of whose balls has lost its connections by destruction of the dumbbells which articulated with it.

The reader will also see numerous vacuoles forming in the following plates and will be able to make identical observations there.

The dumbbells gradually undergo the following alterations under the influence of the fixative: we see certain dumbbell balls very strongly swollen and whose bursting is the mode of destruction. The rods of certain dumbbells gradually lose their affinity for the dye and at the same time they gradually shrink. Many dumbbell rods are in the process of undergoing these two alterations at all points of the cytoplasmic network of the cells in Plate 2. In others, the rod corrodes at a specific point where complete destruction and rupture will subsequently occur.

When a sufficiently large number of dumbbells are thus altered and have lost all resistance, the network disintegrates and the constituent elements are released with shapes that depend on the location of the rupture points.

The existence and location of these breakpoints of the dumbbells of the cytoplasmic network are very visible at the periphery of the vacuoles or on the edges of the external cell wall and around the nucleus in most of the plates in this work.

The origin and significance of vacuoles are also studied very comprehensively later.

3. Evidence of the destruction of the organization of the cytoplasm (cytoplasmic network) in the cells studied and drawn by Guilliermond

This destruction is indisputably proven:

A) By the total absence of a cytoplasmic network in the drawings in which **Guilliermond** depicted the elements of the cytoplasm that he claims are normal and to which he obviously gave the form and location that he believed to be normal; he depicted no trace of the constant connections that link the nucleus to the cell wall. The demonstration of the indisputable existence of the cytoplasmic network and its elementary constitution in the cells of the leaves of *Elodea Canadensis* is made in plates 4 to 9 of this book; the presence of the cytoplasmic network in the cells of these plates constitutes formal proof of the destruction of this network in the cells drawn by **Guilliermond** in his plates since he neither depicted nor described it himself. We will, moreover, accumulate multiple proofs of this destruction.

Figures 1 and 3 of plate 6 of this book are photographic reproductions of figures 4 and 5 of plate 1 of **Guilliermond's** memoir; they are drawings representing, according to him, the cytoplasmic organization and the normal constituent elements of the cytoplasm of young leaves of *Elodea Canadensis*. We note in these drawings:

1. that the cells possess no structure and no trace of a cytoplasmic network;

2. that the various elements represented there are free, without any connection with each other, with the nucleus, or with the external membrane;

3. that these elements consist almost exclusively of granules and filaments (chondrioconts) of varying length; there are only a few dumbbell stumps in figure 3;

4. that the nucleus is free and mobile, without any connection with the external membrane of the cell;

5. that no external membrane is represented, which would mean that the cell does not have one.

In this same plate, I have placed opposite these drawings, photographs (fig. 2, 1100 magnification and fig. 4, 750 magnification) of longitudinal sections of young leaves of

Elodea Canadensis adhering to the terminal bud, fixed with **Ringer's** solution, containing 12% formalin; the sections are stained with ferric hematoxylin; we see that these leaves had the following lengths: that of figure 2, approximately 1.8 mm; that of figs. 4, 1.2 mm; the cells of these leaves are therefore even younger than those of the leaves drawn by **Guilliermond.**

By examining these photographs, we immediately notice that the content and organization of the young cells do not present any point of resemblance with those of **Guilliermond's** drawings. We can distinguish:

1. a large nucleolus showing in certain cells a highly interesting fact: the crown of radiating dumbbells which connect the nucleolus to the nuclear membrane;

2. A large nucleus more or less easily delimited;

3. A membrane separating the cells from one another;

4. Radiating tracts which a careful examination with a magnifying glass reveals to be dumbbells which connect the nucleus to this membrane, either directly or indirectly after articulation with other dumbbells.

The cells of these two leaves are already altered by the fixative, but nevertheless show a certain organization and a characteristic structure that are completely lacking in the cells drawn by **Guilliermond**.

On the other hand, examination under a magnifying glass indicates that the filaments are dumbbell organelles and are not at all arranged as **Guilliermond** figures. But the elements are too close together, too tightly packed in these young cells for us to have a very clear view of them. These facts already demonstrate that the cytoplasmic organization is destroyed in the cells drawn by **Guilliermond**.

The result will be much more convincing when looking at the cells of adult leaves where the cytoplasmic organization is more complete, clearer, and more apparent.

In plate 7, I have taken as a term of comparison with my own observations two figures of cells of adult leaves of *Elodea Canadensis* that **Guilliermond** drew in plate 3 (figs. 5 and 6) of his memoir (10). Figure 1 of Plate 7 of this book is the photographic reproduction of these two cells. Figures 2, 3, 4, 5, of this same Plate 7 are photographs at a magnification of about 1100 of sections of adult leaves from the terminal part of the stem of *Elodea Canadensis*, fixed for 72 hours in **Ringer's** liquid containing 12% formalin. The sections are longitudinal and approximately perpendicular to the surface of the leaf for Figures 2, 3, 4, oblique for Figure 5; the staining was done with iron hematoxylin.

Comparison of the cells drawn by **Guilliermond** with the cells in Figures 2, 3, 4, and 5 immediately reveals in the former (Fig. 1) the total destruction of the cytoplasmic network and in the latter (Figs. 2, 3, 4, and 5), the preservation of this network, already altered, but still very clear in all the non-vacuolated points, a network which connects the nucleus to the external membrane.

This comparison alone constitutes indisputable proof of the total destruction of cellular organization in the cells observed and drawn by ***Guilliermond****.*

The appearance of the large vacuoles, the configuration of their edges, and examination of the points of rupture of the dumbbells in Figs. 2, 3, 4, and 5, Plate 7, and 1 to 5, Plate 8, show that, quite clearly, they are regions of the cytoplasmic network where the dumbbell organelles have been destroyed by the altering action of the fixative.

These vacuoles, absolutely similar to those shown in **Guillier-Moud**'s drawings, are, like the latter, completely devoid of elements because those that existed inside these cavities were destroyed by the fixative.

We can also see the destruction continuing at the periphery of the vacuoles; it is made manifest by the fraying of the elements of the network at the edge of the alveolar cavities where they have a free end which is only weakly colored; the examination of the vacuoles of the cells in figures 2, 3, 4, 5, plate 7, and 2, plate 8, is formally demonstrative in this regard.

In the middle of the already vacuolated part of Figure 1, Plate 8, we see barely colored elements, clubs, dumbbell balls, short or long filaments whose destruction is nearing completion, then on the edges of the cavity, more colored elements in the process of destruction, but a certain number of which are already separated, clubs, dumbbells, etc. Finally, above the nucleus, there is a zone in which the cytoplasmic network still in place is in the process of destruction and dislocation, the elements undergoing a real liquefaction. This cell therefore perfectly shows the evolution of this phenomenon of vacuolation.

In figure 2, plate 8, we will see 2 centimeters above the center of the figure, a group of 4 dumbbells articulated between them by one of their balls and in the process of deterioration; one of these 4 dumbbells, the upper left, is already three-quarters destroyed; there remains little more than the inflated ball which now only carries a very small length of the stick.

In the same plate 8, figures 4 and 5 (1100 enlargement) which are the photographs of cells not far from the terminal meristem of the stem, show that the radiating tracts

which connect the nucleus to the cell membrane and have the obvious purpose of ensuring the fixity and immobility of the nucleus in the middle of the cell are indeed dumbbell organelles; although the cellular organization is almost totally destroyed there, we see, in the cells located to the left and right of the center of Figure 4, the nucleus still attached to the external membrane by radiating tracts. On the right side, we see below and a little to the left of the nucleus one of these tracts consisting of two very distinct dumbbells placed end to end and articulated together very clearly also by one of their balls, another being adherent, either to the nucleus or to the external membrane.

Another similar dumbbell tract, but with a ball a little less distinct, exists at 6 or 7 millimeters to the right of the previous one.

Two other tracts exist to the left of the nucleus located on the left of Figure 5, but they are linked together for part of their course, thus taking on a Y shape.

Before leaving the examination of this plate, let us note in Figure 3 relating, like Figs. 1, 2, 4, 5, to the adult leaf of *Elodea Canadensis,* the new demonstrations that it gives relative to the structure of the cytoplasmic network, to the fixation of the nucleus by radiating dumbbells, to the length of the rods of the dumbbells and to the articulation of these between them by their balls or to the nuclear membrane and the external membrane.

Figures 6 and 7 will not be of concern here, but later. They provide very clear and important demonstrations of the shape of the dumbbell organelles, their mode of articulation, and the constitution of the cell membrane and the external epidermal membrane of the stem.

Plate 9 shows the cytoplasmic network in cells observed in these longitudinal sections fixed with 12% formalin **Ringer's** solution and stained with ferric hematoxylin from:

Figures 1, 2, 3, of adult leaves of *Elodea Canadensis* (1100 gr.).
Figure 4 of a 2 centimeter long potato sprout (750 gr.).
Figure 5 of the radicle of a chestnut sprout (1100 gr.) fixed with **Regaud's** solution and stained with hematoxylin.

These figs. 1, 4, and 5, in particular, show, in different plants, the cytoplasmic network, its destruction characterized by vacuolization and the progressive alterations of the network's elements. Note, on the right of figure 5, a cell in which a shred of the cytoplasmic network still exists, showing the articulation of the balls of the peripheral dumbbells of the cytoplasmic network with the external membrane. This figure 5 demonstrates, at the same time, that with **Regaud's** fixative (Formol-bichromate) used by **Guilliermond**, one can, despite its altering action, detect the cytoplasmic network.

In summary, vacuoles are proof and an absolutely certain sign of the destruction of the cytoplasmic organization and cannot be considered as normal constituent parts of this organization, or of a vacuolar system or apparatus (vacuome).

It has been shown, above that the entire space between the dumbbells and in the meshes of the cytoplasmic network is a single cavity filled by a liquid hyaloplasma which can circulate freely, at all points, from the nucleus to the external membrane.

This large and unique cavity, whose appearance is well represented in the photographs of plates 1 and 2, constitutes in itself the proof of the non-existence of another useless vacuolar apparatus or, if you like, it is itself which is the vacuome, and this only comprises a single cavity, the cytoplasmic cavity.

We could confine ourselves to the multiple proofs given so far of the total destruction of the cytoplasmic organization in the cells examined and depicted by **Guilliermond** regarding *Elodea Canadensis*. But we will still add new evidence relating to the destruction of this organization in the cells of the bean embryo, and then we will provide yet another, which, this time, was provided and presented in a memoir by **P. Dangcard** relating to the cells of the albumen of the castor bean seed.

EVIDENCE FOR THE EXISTENCE OF THE CYTOPLASMIC NETWORK IN BEAN EMBRYO CELLS

In this same memoir by **Guilliermoud** that we have studied so far, this author published, on the subject of a study on the bean seed, figures of the cells of the meristem of the radicle, (fig. 8 to 12, pl. 7, fig. 1, pl. 8) of the cortical parenchyma of the radicle (fig. 2, pl. 8) and of the stem, (fig. 4, pl. 8), of the parenchyma of a bean root (fig. 4, pl. 9), and finally of the cells of the cotyledon (fig. 5 to 13, pl. 9). In all these cells we note:

1. The presence of disparate elements, grains, short rods, more or less long filaments, flexuous with or without dilations, amyloplasts of various sizes, etc., scattered elements, without any connection between them, neither with the nucleus nor with the external cell membrane.

2. The presence of more or less large vacuoles, one of which can occupy more than half the cell volume (Fig. 11, Pl. 7);

3. The total absence of connection between the cell membrane and the nucleus, which is thus floating in the cell;

4. The total absence of cytoplasmic organization.

In accordance with previous findings and conclusions, these characteristics allow us to affirm that the cells of the bean in plates 7, 8, and 9 of **Guilliermond's** memoir had their cytoplasmic organization completely destroyed.

The evidence we will provide to demonstrate this is contained in our plate 9 bis. Figure 1 of this plate is a photograph of figures 4 and 5 of plate 9 of **Guilliermond's** memoir; these two figures represent cells of the parenchyma of the bean root and show the elements and constitution that this author attributes to their cytoplasm. This constitution being identical to that of the other cells concerning the radicle or the stem of the bean which appear in plates 7, 8, 9, of **Guilliermond's** memoir, it is these figures 4 and 5 of plate 9 that we will take as a point of comparison with the photographs of figures 2 to 6 of our plate 9 bis which show the cytoplasmic organization of the various regions of the embryo of the bean seed either at the beginning of germination, (figs. 2, 3 and 4), or in the embryo in the process of formation in seeds having only reached a third or a half of the normal size which they have at maturity (figs. 5 and 6).

The seed is fixed for 72 hours in **Ringer's** liquid containing 12% formalin and then included in paraffin. The sections are stained with ferric hematoxylin. The cells in Figures 2, 3, and 4 are located in the region of the embryo near the insertion of the cotyledon peduncle, and the cells in Figures 5 and 6 are located in the radicle of the embryo, a short distance from the terminal meristem.

Examination of these figures shows, in all of them, the existence of the cytoplasmic network, the dumbbells of which are clearly visible at many points, as well as their mode of articulation.

The balls of the dumbbells are very swollen in the cells of the central cylinder (Figs. 2 and 4) and much less so in the cortical zone (Fig. 3 and upper right corner of Figure 4). At certain points, the radiating arrangement of the dumbbells around the nucleus is clearly apparent. The empty spaces between the cells in Figures 5 and 6 result from their separation when the sections were spread by heating, and this is due to their very small thickness (2 microns or less).

Although in all these Figures 2 to 6 the organization of the cells is already altered by the fixative, the cytoplasmic network has retained such clarity that it will certainly appear to the reader that any discussion is rendered superfluous on the evidence of the destruction of this network in the cells drawn by **Guilliermond** and on the fact that the disparate and shapeless elements that he depicted in their interior are the scattered debris of this network that he described as being the normal forms of mitochondria.

Thus, once again, it is also demonstrated, with evidence that removes any possibility of discussion, that *mitochondria as Guilliermond and others have described them, do not exist and that they are only the scattered and shapeless debris of the dumbbells of the destroyed cytoplasmic network.*

We will conclude these multiple demonstrations with the presentation of that provided by **P. Dangeard** on the destruction of the cytoplasmic network in the cells of the castor bean albumen studied by **Guilliermond**, a destruction that escaped the latter.

PROOFS OF THE EXISTENCE OF THE CYTOPLASMIC NETWORK IN THE CELLS OF THE CASTOR BEAN ALBUMEN

Plate XIII of **Guilliermond's** memoir (10) relating to the embryo and albumen of the castor bean seed, shows cells whose cytoplasmic organization has been completely destroyed (figs. 1, 2, 3, 4, 6, 7). Figure 5 shows the shapes of the debris of the dislocated cytoplasmic network, among which are found different forms of plastids whose nature and origin are studied later.

Pierre Dangeard, who studied the constitution and evolution of the cells of the embryo and albumen of the castor bean seed (3), contested the accuracy of some of **Guilliermond's** observations. He made the following interesting observations:

1. He noted in the albumen of the castor bean seed from the time immediately preceding maturity until complete maturity, the existence of an alveolar protoplasm with very fine meshes, and he specifies on this subject:

I have already written on several occasions about this very curious arrangement of the cytoplasm: it corresponds quite well to the old descriptions of ***Mr. Beauverie.*** *On the contrary, it remained unnoticed by* ***Mr. Guilliermond****. It follows that the latter, having expressed the opinion that I had been able to make imperfect stainings precisely in this case, could not hold it against me, since he appears, for his part, to have made an incomplete fixation.*

This simply means that if P**. Dangeard** noted that **Guilliermond** did not see this alveolar network in the Ricin albumen cells that he examined, it is because it no longer existed there, having been destroyed by fixation and other preparatory operations;

2. He also noted the absence of the long undulating filaments described by **Guilliermond** first in the albumen of the ripe seed and finally in that of the germinating seed.

This means, obviously, that the presence of these long filaments is due to the same cause that destroyed the cytoplasmic network; therefore, since **P. Dangeard** does not see long undulating filaments when he sees the alveolar network, and since **Guilliermond** sees

these long filaments when this alveolar network no longer exists, since he did not see it, it is therefore because they result from the destruction of the network and are its scattered debris. **P. Dangeard's** observations therefore indirectly provide a new demonstration of the nature and origin of these long filaments, a demonstration already made many times and by other means in the course of this work;

3. P. Dangeard only noted the existence of the cytoplasmic alveolar network in the period of maturity and immediately before germination. But he no longer observed its presence in the very young states of the albumen and before maturation, at the same time as this absence, he observed the presence, at the very beginning of the formation

of the albumen, of the long flux filaments observed by **Guilliermond**. This is therefore clear proof of the accuracy of the conclusion drawn above: these filaments are the scattered debris of the destroyed cytoplasmic network.

Another proof of this destruction is provided by the presence of the enormous vacuoles which we notice in the cells and which occupy (fig. 1 page 462 of the afore-mentioned memoir) approximately three quarters of the cell.

These enormous vacuoles exist both in the figures in **P. Dangeard's** note, particularly Fig. 1, and in the figs. in Plate 13 of **Guilliermond's** memoir. In Figure 4 of this plate, there is only a single central vacuole that occupies three-quarters of the cell, the cytoplasm of which is reduced to a narrow peripheral band in which the nucleus has been rejected, which also attests to the destruction of the central part.

In the older, but still very young, stages of the endosperm cells, **P. Dangeard** observed only grains and short rods in the cytoplasm. If he did not observe the existence of the alveolar network, it is because it was destroyed, these grains and short rods being its scattered debris. **P. Dangeard** draws the following conclusion from his observations:

During the beginning of germination the cytoplasm retains this reticulated structure that it had in the mature seed but, after a certain time, the contents of the alveoli no longer dissolve as they did before; they remain in place on the preparations in the form of a yellowish or grayish compound which slightly retains the hematoxylin. Nevertheless, the cytoplasmic reticulum retains almost the same characteristics and its regularly polygonal arrangement.

*During this entire germination period, **M. Guilliermond** completely neglected this description of the fundamental cytoplasm; on the other hand, he figures various chondrioconts with or without vesicles. These chondrioconts could perhaps come from an alteration of the cytoplasm, because I have never observed them. As I have represented in Figures 5 and*

6, relating to the germination of Castor Oil Plant, all the cytoplasmic elements then appear as grains or short rods.

P. Dangeard's hypothesis is therefore in accordance with reality, but it must be added that the grains and short rods whose existence he himself noted and which are shown in figures 5 and 6 of his memoir, are all linked to each other in the cytoplasmic network which they constitute, while in figures 1, 2, 3, these grains and rods are separate, distinct and unrelated to each other.

Figure 4 of plate 15 of this book contains the photographs of figures 3 and 4 of **P. Dangeard's** memoir which show these different aspects.

This same plate contains in figures 2 and 3 photographs of cells from the albumen of the castor oil plant (fig. 2 and 3 fig. 3) or from the cotyledon (7 fig. 3) contained in plates 13 and 14 of **Guilliermond's** memoir (10). The cell in fig. 2 shows in particular an enormous central vacuole and a total destruction of the cytoplasmic network attested by the shapes of the scattered debris of this network and by the position of the nucleus, rejected to the peripheral part of the cell following the destruction of all the connections which fixed its position in the central region of the cell. The two cells in fig. 3 show the same total destruction of the cytoplasmic network.

One might be surprised that **P. Dangeard** did not observe in the cells of young albumen the presence of the cytoplasmic network that he observed in the cells of mature albumen or during germination, but this fact is explained by the following observation:

Sections of embryos during germination show that the cells of the terminal meristem and of the entire end of the radicle have a cytoplasmic network much more sensitive to the destructive action of fixing liquids than the albumen; the latter resists and persists quite easily.

There are, moreover, considerable differences between species in terms of this resistance. At least partial conservation of the cytoplasmic network is fairly easily obtained in the radicle of the bean, the horse chestnut, and the oak acorn, while the network of the radicle of the Ricin seed and most Cucurbitaceae is very fragile and very susceptible to deterioration.

In short, this presentation shows that, when the cytoplasmic network is intact, there are neither long flexuous chondrioconts, nor short rods, nor isolated granules in the cytoplasm. These different elements are only found when it is destroyed. When the cytoplasm is intact, all its elements, which are of a single species, the dumbbell organelles, are included in the cytoplasmic network.

To conclude this question, let us add that the cytoplasm is not in reality alveolar; it is not formed by closed and separate alveoli; the appearance of figures 4, 5, 6 of **P. Dangeard's** memoir could lead one to believe this. The polygons which show these figures are in reality often incomplete, certain sides not being in the plane of the polygon itself and the dumbbells which constitute them are articulated with a neighboring polygon situated in another plane. On the other hand, the interior of all these polygons is not closed by a membrane, it is a free space occupied only by the cytoplasmic liquid, the internal liquid medium of the cell which fills the entire free cytoplasmic space, a space which forms only a single cavity whose points all communicate with each other and in which the liquid can circulate everywhere freely. Only the polygons that constitute the external cell membrane and which are all located on the same curved surface are or appear to be closed by a membrane that isolates the cell from its neighbors.

Thus the vacuome, consisting of a group of cavities isolated from each other, does not exist and is an erroneous conception. There exists only the total, unique cytoplasmic space, forming a single cavity filled by the internal liquid medium of the cell which circulates freely and in which soluble bodies coming from the outside can diffuse everywhere in a centripetal direction and in a centrifugal direction those which emanate from the nucleus and the cytoplasm.

This is not a personal and theoretical conception; it is an easily verifiable fact. To do this, it is enough to place, for example, between slide and coverslip a leaf of *Elodea Canadensis* with a drop of the water in which the plant lives and to apply light pressure to the coverslip with a pointed instrument, which sets the chloroplasts in motion, making them travel throughout the entire cytoplasmic space, which proves that circulation is free everywhere. This fact, which can be verified on the leaves of all plants and under conditions where the cytoplasm remains strictly intact and unaltered, formally opposes the notion of a vacuome constituted by multiple cavities of the cytoplasm, limited by a proper wall and isolated from one another.

The constitution of the cytoplasmic network also opposes this notion since it is spread uniformly throughout the cytoplasm and the vacuoles that have been considered as the constituent elements of the vacuome are only the points where this network is destroyed by the preparation processes of the tissue examined, that is to say by the fixative, by the liquids used during inclusion, and then subsequently by the operations intended for staining; the numerous photographs of these zones of destruction of the cytoplasm that this work contains are so convincing that it is unnecessary to insist further on this question.

4. Nature and origin of the scattered elements, of varied shape and without any connection between them, that Guilliermond considers to be chondriosomes.

It results from the examination of the photographs in figures 2, 3, 4, 5 of plate 7 and the figures in plates 8, 9, 10, then from the previous knowledge that we have of the cytoplasmic network, that in the part of it occupying the non-vacuolated points, all the organelles constituting this network are linked and articulated between them by their balls. The figures in plate 8 also provide beautiful demonstrations of this.

Now, in the cells of figure 1, plate 7, drawn by **Guilliermond,** the elements that he considers to be the normal constituents of the cytoplasm are isolated, independent, without any connection between them. Since they are there where the cytoplasmic network should exist, but no longer does, having been altered and dislocated, there is no doubt that these elements are its scattered debris and that this is the reason why they no longer have any connection between them and have bizarre forms which one could say are infinitely varied; this variation in form is explained by the fact that it is due to the random position of the breaking points of the elements of the dislocated network. If one considers that the alteration by the fixative has not only resulted in dislocating the network, but also in modifying the dimensions of the various parts of the dumbbell organelle, either the size of the rod which can be swollen or reduced to the state of a thin filament, or the size of the balls which can be greatly swollen, one can easily explain all the forms of the isolated elements which **Guilliermond** has shown in his drawings.

These forms can be reduced to the following:

A) **Granulations.** These are either residues of elements, residues of chondriomites, or detached dumbbell balls.

B) **Short rods**. These are either fragments of rods or whole rods of all lengths.

C) **Long rods, straight or arched**. In the first four plates, we will see long, straight or arched rods belonging to a single dumbbell; several dumbbells placed end to end sometimes give the impression of a single filament; only a clear photograph allows this point to be judged; as an example of this fact, neurofibrils that have the appearance of straight and continuous filaments are made up of dumbbells placed in a row.

D) **Bifurcated filaments without expansions**. An example of this can be seen in Fig. 9, Plate IV of **Guilliermond's** memoir in the form of an oddly shaped, twice-branched element. This element must have carried refracting balls that **Guilliermond** did not see, at the so-called branching point because dumbbells do not branch.

E) **Clubs.** These are dumbbells from which one ball has fallen, or a half-dumbbell; sometimes the ball of a neighboring dumbbell remains attached to the ball of the half-dumbbell.

F) **Rods or filaments with several balls along their path**. **Guilliermond** calls these balls dilations which are, according to him, the beginning of the evolution of a chloroplast or an amyloplast. They are simply dumbbells placed end to end coming from two or three neighboring disorganized meshes; such a filament can also come from a single mesh whose edge is unrolled. What affirms this origin is the fact that, in certain cases, the dilation comprises two joined balls which, frequently, have been indicated by a transverse line dividing an oval dilation in two; there are oval dilations formed of three balls separated by two transverse lines, which indicates that such a dilation was, at its location in the cytoplasmic network, a point of articulation of the balls of several dumbbells. As an example of this fact, we will see, to the right and below the nucleus, in the right cell of Figure 5 of Plate IX (bean) of **Guilliermond's** memoir, an element of bizarre shape, convoluted, branched, bearing four dilations, one of which, oval, is segmented by two transverse lines which indicate three balls placed side by side. Such an element is most characteristic for demonstrating the origin, indicated above, which its shape and its characteristics make manifest; two other examples of this same fact are visible immediately below this element.

G) Other elements drawn by **Guilliermond** are much larger, have the shape of round or pyriform vesicles or are more or less regularly oval and are equipped with a short filament-shaped appendage or appendage of variable length (clubs) or two appendages with an irregular insertion point.

These elements are, according to **Guilliermond**, plastids (chloroplasts or amyloplasts) in the process of formation. Each of these two categories of elements, of particular importance, will be studied further and separately.

To fully convince the reader of the nature, origin, and form, as well as the relationships of these various elements to each other and to the nucleus and nuclear membrane, I have brought together in plates 10 and 11 the most diverse images or tables of cellular destruction in the terminal bud of *Elodea Canadensis*, which he can study and compare with the numerous cell figures drawn by **Guilliermond.**

He will be able to convince himself that the appearance of the latter and the arrangement of their elements do not correspond, and in no case, to those observed in the photographs of cells in plates 10 hes 10 and 11. He will see that always, even in the most altered cells, the nucleus remains connected to the cell wall by a certain number of

tracts or links (dumbbells) which, normally, ensure the fixity of its position in the middle of the cell and which, for this reason, affect a radiating arrangement; when these links are broken, we find the remains adhering to the nucleus or to the cell wall. *But this device of capital importance has totally escaped Guilliermond who, in all the figures of his memoir has never shown even a single tract connecting the nucleus to the wall and has never mentioned in his descriptions*. By examining the figures on plates 7 and 8 of this volume with a magnifying glass, the reader will be able to observe in the most diverse situations and states, the remaining elements in the cell in the process of destruction;

he will, thus, be able to observe their progressive liquefaction under the action of the fixative and acquire the conviction that all the visible and observed elements can be reduced to a single type: the dumbbell organelle of which they are either a normal element, or only a part, or even an assembly articulated by the balls.

He will also note that these elements are not placed in a disordered fashion, in any position, as **Guilliermond** depicted in his drawings, but that they maintain relationships between them which are significant, constant and not the effect of chance.

From this study we will conclude:

1. *That the scattered, isolated, and variously shaped elements drawn by Guilliermond are debris from the altered cytoplasmic network, dislocated into tiny fragments;*

2. That the varied shapes of these elements are due to the chance of dislocation and breaking points of the network;

3. That these elements are all decomposable into altered dumbbell fragments, or into network fragments;

4. *That these various characteristics are new proof of the destruction of the cytoplasmic organization in the cells studied by Guilliermond;*

5. *That these elements described as normal elements of the cytoplasm and the chondriome called mitochondria do not exist if we consider them with the firmness and the properties that have been attributed to them.* In the cell, there exists only the dumbbell organelle, which is never free and is always included in the cytoplasmic network;

6. That, consequently, these elements, which result from the dislocation of the cytoplasmic network in the process of being destroyed by the altering action of the fixative:

A) Did not exist in the same form in the normal plant when it was immersed in the fixative.

B) *These were elements that were already altered, in the process of being modified and destined to disappear at the moment when the fixation of the plant was stopped.*

C) And were consequently wrongly considered by **Guilliermond** to be in the process of evolution for the formation of plastids.

In the course of the study of the nature and origin of chloroplasts and amyloplasts, or more generally of plastids, which we are going to undertake, we will demonstrate by examining the text of **Guilliermond's** memoir and by studying his drawings that his conclusions relating to the evolution of chondriosomes and especially of chondrioconts to form chloroplasts and amyloplasts are unfounded.

The structure of the cytoplasm that I indicate here has been completely confirmed in animals by the study of liver cells and adrenal capsules in which plate 26 of this volume shows it almost schematically. This schematic structure is represented in fig. 1 page 74.

IV. STUDY OF THE SHAPE, ORIGIN, AND STRUCTURE OF CHLOROPLASTS

They have been studied by a series of authors.

A. Meyer (48) observed small, slightly green, round or oval granules in the cells of leaf primordia measuring 0.7 to 1 mm. According to **Mikosch** (18), young chloroplasts are fusiform.

Lewitsky (41) studied the origin of chloroplasts in very young leaf primordia (0.8mm) of *Elodea Canadensis*. According to him, they come from dumbbell-shaped chondriocontes; as they grow, they release the young chloroplasts, which usually have an oval shape. **Guilliermond** (27), who analyzed **Lewitsky's** memoir, considers this origin of chloroplasts to be well demonstrated However, a careful and detailed study of the latter's memoir leads us to the certainty that he did not provide this demonstration.

The photographs which accompany his memoir, one of which is reproduced very precisely in plate 12 (fig. 2) lack clarity and do not provide any demonstration of his conclusions; there is nothing there to support the demonstration of the transformation of chodrioconts into chloroplasts. On the contrary, at certain points (in particular the upper left corner) the demonstration of the granular constitution of the chloroplast, explained later, a constitution which implies a completely different origin.

It is understandable, moreover, that this demonstration is not possible since, from all the parts of the current work, it results that the chondrioconts are the middle part of the dumbbells and that these, whose exclusive function is the formation of cellular elements and tissues, **are never free in isolation** and are always articulated with other dumb-

bells to form either a network or a filamentous element when they are articulated end to end; the chloroplasts are therefore of a nature completely different from that of the dumbbell organelles.

Sapchine (73) also admits that the chloroplasts derive from the chondriosomes which would divide, taking the form of dumbbells. Later, these dumbbells grow and become chloroplasts. Here too, the conclusions are mainly hypothetical and not supported by precise and verifiable facts.

Guilliermond indicates (loc. cit. p. 10) that he conducted short studies which confirmed **Lewitsky's** results and showed that chloroplasts derive from typical chondrioconts. But he does not present any precise facts on this subject, and provides no proof. Moreover, the intimate constitution of chloroplasts, which will be explained later, constitutes a definitive demonstration of the inaccuracy of **Lewitsky** and **Guilliermond's** conception.

Konrad Noak (54), on the other hand, concludes that the chloroplasts of the living cells of the bud meristem of *Elodea Canadensis* have a rounded shape and are distinguished from ordinary chondriosomes by their larger size, their green color and their microchemical characteristics; and he clearly states that they have no resemblance to chondriosomes and that they never have the form of chondrioconts. The results of my personal observations are in accordance with **Konrad Noak's** conclusions.

Before examining the constitution and origin of chloroplasts myself, I will examine the observations made by **Guilliermond** on this subject. On pages 22 and 23 of his memoir (10), he states that "*the bud of Elodea Canadensis constitutes one of the most favorable objects we know for the demonstration by mitochondrial methods as well as by vital observation of the formation of chloroplasts from chondrioconts. Fixations succeed much more easily than in most other buds...*, etc." I have noted, on the contrary, that the buds of *Elodea Canadensis* are extremely fragile in the presence of fixatives, all of which alter the cytoplasmic structure and, for the most part, completely destroy it.

Clear proof of the accuracy of this statement is provided by **Guilliermond's** own drawings, which show, peremptorily, that the cells drawn by him have their cytoplasmic organization completely destroyed.

Here is how **Guilliermond** describes the formation of chloroplasts in the leaf primordia of the bud of *Elodea Canadensis*; he writes on page 17, 9th line:

In the leaf primordia, where chloroplast differentiation takes place at the expense of chondrioconts, the granular mitochondria increase in number and at the same time slightly decrease in volume, then they show a tendency to take on the appearance of short rods.

Finally, in cells where chloroplast differentiation is completed, the majority of them continue to elongate and become typical chondrioconts, very thin, sometimes branched.

Let us also note, on page 16, line 9, the following passage:

The chondrioconts thicken slightly (0.6 microns) (pl. I, fig. 4 to 6, and pl. II, fig. 2 to 8) then, if they are not very elongated, they transform entirely into ovoid chloroplasts or rod-shaped ones, or if they are very elongated, which is the most frequent case, they form swellings along their path which are isolated by breaking the tapered parts which unite them, in the form of rounded corpuscles This transformation takes place in the region between points A and B of our figure I. The small chloroplasts thus formed gradually grow and become typical chloroplasts in the state of large rounded corpuscles, in the form of spindles, clubs or rods (3 microns in diameter) (pl. I, figs. 7 and 8, and pl. III, figs. 1 and 2). These can continue to grow and divide in cells that have reached their full development, and we find there numerous dumbbell-shaped chloroplasts which represent stages of chloroplast division.

All the statements made by **Guilliermond** in these two quotations are imaginary or hypothetical and can be described with certainty as follows:

1. Because they relate to elements that have no real existence and which, contrary to what **Guilliermond** believed when writing his memoir, are not normal or autonomous elements.

They are scattered debris of the destroyed cytoplasmic network, stumps of dumbbell organelles or several united stumps of these dumbbells;

2. Because these elements, debris of the deeply altered, disorganized, and dislocated cytoplasmic network, were not destined for constructive evolution, but for definitive destruction if the fixative, which brought them to this state, had continued its action.

This being rigorously established by **Guilliermond's** own drawings and by the demonstrations set out previously, the significance and value of all the modifications of an evolutionary nature that he attributed to these altered elements, scattered and shapeless fragments of the dislocated cytoplasmic network, are purely imaginary; **Guilliermond** attributed an evolutionary nature, and without plausible reason, to forms that are due exclusively to the altering action of the fixative liquid on the dumbbell organelles (swelling, liquefaction) and to the randomness of the breaking points of the meshes of the altered cytoplasmic network during its dislocation.

In addition, there is an inaccurate statement in the last four lines of the last quote: *there are no free dumbbells in a normal cell,* and there are no green dumbbells in either normal or fixed cells; chloroplasts are never shaped like dumbbells.

On the other hand, one notes in reading **Guilliermond's** memoir that, when he asserts the transformation of a granulation into a chondriocont, the thickening of a chondrioconte or its transformation into an ovoid chloroplast or a rod, he does not provide the slightest element of proof; he bases his deduction only on the existence of mitochondria in the form of round grains, or very short rods, or slightly longer rods, or even longer filaments, or similar filaments provided with 1, 2, 3, dilations along their path, or finally larger vesicles. And he established between these disparate elements an evolutionary filiation that is purely imaginary. The elements have no relationship to each other other than that which they have in the normal cytoplasmic network from which they originate.

It is striking to note that, at various points in his memoir, **Guilliermond** indicated that dilations, either club-shaped and isolated, or located on the path of chondriocontes scattered throughout the cell, will become either amyloplasts or chloroplasts, and this without indicating by what sign he recognizes in advance that, among two dilations of the same size and which nothing distinguishes one from the other, one will become a chloroplast and the other an amyloplast.

The astonishment increases further when we read a page further (p. 17, 23rd line):

Sometimes, we also encounter vesicular grains or chondrioconts provided along their path with a small vesicle; it is difficult to know whether these vesicles result from an alteration or represent a functional state of the chondriosomes.

Now, the vesicles he speaks of here are the same as those that, above, could become either chloroplasts or amyloplasts, without the reason being explained. I also recall that it was explained on page 49 that the dilations or vesicles that we notice on chondrioconts are the balls of the dumbbell organelles, isolated or placed end to end and inflated or not by the action of the fixative.

The last quote is enough to show the fragility of **Guilliermond's** conclusions regar- ding the evolution of mitochondria. The fact that these have no real existence either as independent, isolated elements, or in the forms attributed to them, removes the question of their evolution and it demonstrates by itself that **Guilliermond's** conclusions on the transformation of granular mitochondria, then of chondrioconts in chloroplasts are inaccurate. We will also research their origin and nature.

* * *

The study of chloroplasts can hardly be done except on the living cell, in the epidermal cells of the leaves or the stem, because the chloroplasts which are very fragile are almost always destroyed in the parts subjected for a long time to the action of fixatives. **Gulliermond** indicates, as a method of vital study of the normal elements of the cytoplasm, the crushing of the terminal bud of *Elodea Canadensis* between two glass slides and the microscopic examination of the elements thus made visible by the crushing. It is hardly necessary to say that this method is contraindicated for the study of elements as delicate as those of the cytoplasmic network, if one wishes to have an exact notion of the relationships which they have between them. It is moreover not useful to use this method because, unlike the cytoplasmic network, chloroplasts lend themselves very well to observation in the living cell.

Three points are of particular interest to us in this study: the shape of the chloroplasts, their structure, and their origin.

Shape of chloroplasts. - It is indicated as round. According to **Lewitsky** and **Guilliermond**, young chloroplasts have an oval shape, and some chloroplasts even have a dumbbell or simple rod shape in the adult cell; we have just proven above the inaccuracy of this last statement.

The shape of the chloroplasts can be easily appreciated; a leaf of *Elodea Canadensis* being detached, it is placed between slide and coverslip between which a drop of the water in which the plant lives has been placed. A field of epidermal cells being developed, one gently presses several times in succession on the coverslip for a second with a pointed instrument and one immediately sees the chloroplasts start moving and move slowly along the edges of the cell. Almost all of them appear regularly round, but at a given moment, they appear oval, because, visibly, they move against the lateral wall of the cell perpendicular to the plane of the coverslip; arrived at a corner of the cell, most of them tilt to penetrate into the depth of the cell and immediately show themselves in the oval shape that they have when seen in profile. When they do not touch the lateral walls of the cell and they are applied against its upper surface, parallel to the plane of the lamella, they are all round.

This means that chloroplasts are round bodies, but flattened; seen from the front, they are round and seen from the side they are oval. Their thickness, seen in this position, is approximately 3 or 2 times less than the diameter of the round faces. Thus, it is not surprising that we observe an oval shape in young chloroplasts since this is the normal shape of the adult chloroplast seen from the side.

This shape of the chloroplasts is clearly visible in Figure 3 of Plate 12 which is a photograph of an intact leaf of *Elodea Canadensis* at a magnification of about 550; by exami-

ning the edges of the cells in the center of the figure, then a little higher and a little lower, the reader will see the oval shape of the chloroplasts pressed against the lateral walls and the regularly round shape of the chloroplasts pressed against the upper surface of the cell and seen from the front. The same observation is made in the epidermis of the leaves of other plants, ivy for example, where they are even more flattened than those of *Elodea*.

Structure of Chloroplasts. - This structure appears very clearly in Fig. 4 (pl. 12), which is a photograph of a fresh, adult leaf of *Elodea Canadensis* examined between a slide and a coverslip in the water in which the plant lives, and after staining for 20 to 30 minutes in the water in which the plant lives, with the addition of a few drops of **Zichl's** fuchsin diluted with 95° alcohol (magnification approximately 1,100) **(1)**.

This staining, which is light, nevertheless allows us to observe with the greatest clarity, as shown in the photograph in Fig. 4, that chloroplasts are bodies formed by the union and adhesion of a certain number of fine spherical granules. Note that, in this intact cell, the cytoplasmic framework and the elements that constitute it remain completely invisible.

This granular constitution of the chloroplasts shows once again the error in the conception of **Lewitsky** and **Guilliermond**, which makes them come from chondrioconts. This error is all the clearer and more certain since, even in the figures of **Lewitsky's** memoir, carefully examined under a magnifying glass, one can distinguish the granular constitution of the chloroplasts (upper left corner of fig. 2 pl. 12 of this book).

In fig. 1 of plate 12, I have reproduced fig. 6 of plate 2 of **Guilliermond's** memoir (10), representing two cells which, according to the legend, would be cells of young leaf primordia in which one could follow the phases of the transformation of chondrioconts into chloroplasts; it has been demonstrated previously that this statement is inaccurate and that the shapeless and scattered elements contained in these cells are the debris in the process of necrosis of the altered and dislocated cytoplasmic network.

In Fig. 6 of Plate 6 of his memoir, **Guilliermond** depicted, in the cells of young leaves, green filaments or chondrioconts, which he says are impregnated with chlorophyll.

Like **Konrad Noak** (54), I have never seen green filaments in cells; moreover, it has been proven previously that free filaments exist only in cells whose organization is destroyed by the fixative and that they come from the dislocated cytoplasmic network.

(1) *Before examining them, these leaves must be carefully brushed with a fine brush under a stream of spring water to detach from the epidermis the foreign elements, bacteria, microscopic algae which are attached to it.*

As there are no free filaments in the living and normal cell, there cannot be green filaments there either. Direct and rapid examination of the cells of an *Elodea Canadensis* leaf not subjected to the action of a dye shows this granular constitution of the chloroplasts.

Figure 5 is a photograph of a fresh adult *Elodea canadensis* leaf examined without any staining between the slide and the coverslip in the water that is its natural habitat (magnification 550). Some of the chloroplasts seen in the cells show for the most part the same granular constitution (although much less accentuated) as those in the previous figure. This granular constitution can therefore be observed even in normal chloroplasts that have not undergone any staining. This granular constitution becomes more apparent when the *Elodea leaf* has been under the objective for more than half an hour between the slide and the coverslip.

Let us also point out that, in these living and intact cells, the nucleus and the granules it contains are sometimes clearly distinguishable. We will see elsewhere that these granules are the external balls of the radiating dumbbells constituting the supporting framework of the nucleus.

In the upper cell of Figure 5, pl. 12, we can even distinguish the nucleolus and, at its periphery, a crown of white granules which appear to correspond well to the internal balls of the radiating nuclear dumbbells and of which one is pressed against the nuclear membrane and the other included in the nucleolus, a device explained later on the subject of the constitution of the nucleus and the nucleolus.

When examining the leaves of Elodea Canadensis under high magnification, we quite frequently see a microscopic green alga, Protoderma viridae, growing on the epidermis. These cells contain chlorophyll grains of a much darker green than the chloroplasts of *Elodea* seen in the same field. These chlorophyll grains, which are spherical and 0.6 to 1 micron in diameter, are either isolated or placed end to end in twos, threes, or fours, and thus simulate short rods of an intense green in which the eye can clearly distinguish the component grains.

This shows that chloroplasts are formed from elementary grains of chlorophyll grouped together in such a way as to form a body which, in most plants, has the appearance of a raspberry and which, in others, takes the most varied forms; in certain algae, for example: *Sphoeroplea, Mesocarpus, Closterium, Spirogyra, Zygnema*..., etc.

These chlorophyll grains are therefore not homogeneous, contrary to the opinion of **Guilliermond,** who says in his memoir (p. 21, 28th line): "*They present very clear signs of alterations which are reflected in the fact that they are not homogeneous as in the living*." Let us add that this lack of homogeneity of the chloroplasts is even noticeable in the

photographs of **Lewitsky** (16) taken on the living and which **Guilliermond** considers so demonstrative.

We note at the same time that the chlorophyll grains isolated from *Protoderma viridae* have exactly the same size as the elementary grains which constitute the chloroplasts of *Elodea Canadensis*. These are therefore, as clearly shown in figure 4 of plate 12, compound elements formed by the union of a fairly large number (at least 30 to 40) of elementary chlorophyll grains joined together.

These elementary grains are composed of a hyaline material containing a chromatophilic substance; some granules are strongly colored, others less so, and some not at all, a fact that can be attributed more to the imperfection of the coloring process than to a difference in the constitution of the grains.

In **Van Tieghem's** treatise on Botany, fig. 344 on page 511 shows, according to **Schimper,** in the pith of the stem of *Vanilla planifolia*, according to the following legend which accompanies it:

*In **A**, young oval chlorophyll grains, arranged around the nucleus, each producing, in all their mass, a large number of small starch grains; in B, the same older grains become spherical and almost completely invaded by starch grains; in C, the green substance has disappeared and each green leucite is replaced by a compound starch grain.*

There is certainly an error of observation here because four chloroleucites (or chloroplasts) shown in C are exactly similar to those in figure 4 of plate 12 of this book and these, having the shape of a raspberry seen from the front, were clearly and strongly tinted green, that is to say grains of chlorophyll; there can be no doubt about this. The same can be said of the chloroleucites shown in B, in **Van Tieghem's** figure, which also have the shape of a raspberry and which would be almost completely invaded by grains of starch, which means that certain granulations would be chlorophyll and other starch. Finally, the young oval chloroleucites shown in A (fig. 344 of **Van Tieghem**) have the same granular constitution as the others.

There are therefore clearly errors of observation or interpretation. **Van Tieghom** indicates that the fine green granules that constitute chloroleucite can only be colored blue by iodine as long as their size does not fall below 0 mm. 001.

However, the elementary granules of the chloroplast all have a size of approximately 0.5 to 1 micron, and in young, adult, or old leaves, none of them are colored blue by iodine.

If, in order to color these granules, it is necessary to first treat them with alcohol to remove the green tint, then with a potash solution which causes the elementary granu-

les to swell, and finally to neutralize them with acetic acid before applying the iodine, the coloring thus obtained loses all meaning due to the chemical modifications that such a treatment may have brought to the granules and also due to the fact that since the granules are all discolored, one can no longer recognize among them those which previously possessed the green tint.

It is indeed true, as **Konrad Noak** has stated, that chlorophyll grains have different microchemical characteristics from mitochondria.

In any case, the young or adult chloroplast of *Elodea Canadensis* is therefore a group of elementary chlorophyll granules, these being, in short, the origin of the formation of the chloroplast; only the origin of these elementary granules remains unknown.

Here we have a direct demonstration which is sufficient in itself to establish that chloroplasts do not come from evolved chondrioconts and I recall here that I have already established, and without possible dispute, that the chondrioconts that **Lewitsky** and **Guilliermond** presented as being in evolution to transform into chloroplasts or amyloplasts, are in reality only the scattered debris of the cytoplasmic network of cells whose organization is destroyed by the action of the fixative and that these debris are in the process of total destruction and not in the process of creative evolution; with even more precision it has been demonstrated that what these two authors took for young chloroplasts are the balls of the dumbbells of the altered and dislocated cytoplasmic network.

Thus by two different ways are demonstrated, on the one hand, the constitution and the origin of the chloroplasts and, on the other hand, the error of those who considered them as evolved chondrioconts.

V. NATURE AND ORIGIN OF AMYLOPLASTS

We have just seen previously, while discussing **Guilliermond's** ideas on the origin of chloroplasts, that, according to him, this is the same for amyloplasts. These would come, like chloroplasts, from the evolution of the dilations that chondrioconts present along their path. We have seen that **Guilliermond** does not indicate by what characteristic he recognizes that the same element (chondriocont) can develop sometimes into a chloroplast, sometimes into an amyloplast.

We have also seen that the dilations of the chondrioconts, which **Guilliermond** considers to be the beginning of this development, are in reality of a very different nature. They are made up of the round balls of the dumbbells, organelles constituting the cytoplasmic network; these dilations are round when they are unique and placed at the end of the rod of a dumbbell; they are oval when they are placed on the path of a filament because in this case, it is formed by two or more complete or fragmented dumbbells joined, as

always, by their balls; there are two balls and not just one in a clear and non-emphasized oval dilation; this quality is also shown in certain drawings of **Guilliermond's** memoir where the oval dilation has a median line which separates it into 2 parts, in figure IV for example, (A, lower right corner). We have seen on the other hand that neither chondrioconts nor their dilations ever present the green tint of chloroplasts.

These various conceptions and interpretations of **Guilliermond** have been demonstrated to be inaccurate by very different methods and, finally, by the study of the structure of chloroplasts, which is incompatible with the origin and evolution he indicated.

Amyloplasts having, according to the latter, the same origin as chloroplasts, their study will provide us with a new demonstration of the error of these conceptions.

Let us examine the different forms and characteristics attributed by **Guilliermond** to the amyloplasts. In plate 14 of this book, we will find, drawn by him, their principal forms gathered in figs. 5, 6, 7, 8, 9, which are photographic reproductions of figs: 8, plates 2, 3; figure 10, page 79, and A, D; figure IV, page 41, of **Guilliermond's** memoir (10). The reader will also be able to examine with interest figures 8 and 10 of plate 4 of this memoir, as well as the numerous others where the transformation of chondrioconts into amyloplasts is represented.

According to these figures, (see plate 14 of this book, figs. 5 to 9):

1. Amyloplasts generally have (figs. 6, 7, 8) an oval shape, sometimes round;

2. They carry, on a part of their periphery or on their entire circumference, one or more thickenings or very chromatophilic filaments which extend at one or x points in the form of a more or less long tail; sometimes this tail ends in a ball; other times, there is a tail on one side and a regular external ball on the other side (upper left corner of fig. 7);

3. Two or even three plastids may be joined together, bearing a common thickening on the same side; three plastids joined together on the left side of figure 6 show, in the space separating the two upper plastids, that the thickening is in reality a filament joined to the plastids; the tails of the plastids are also and very clearly the continuation of the filaments joined to the plastids. We also note in figure 5 against the nucleus and below, two plastids joined together by a tract that is smooth and short or longer and arched (upper right corner of fig. 8, pl. 14);

4. Compound starch grains are made up of several plastids joined into a single more or less regularly round mass, whose periphery and separation lines are thickened at various points; sometimes, these compound grains bear one or more tails like simple plastids: **Guilliermond** (fig. VIII, i, p. 71).

Let us add to these characteristics that these plastids contain starch and that, when placed in contact with an iodine solution, they take on a special coloration.

We will now examine these plastids in the same preparations of terminal or axillary buds of *Elodea Canadensis* that we used to study the cytoplasmic network; I recall that the fixation of these buds was carried out with 10% formalin (25 cc. of 40% formalin, added to 75 cc. of water) or with 10% formalin **Ringer's** solution and staining with ferric hematoxylin. Plates 13 and 14 (Fig. 1, 750 magnification, Figs. 2, 3, 4, 5, 6, 1,100 magnification) contain photographs of cells from a region very close to the terminal meristem of the stem, presenting a very suggestive picture of the destruction of plant tissues by fixatives, the examination of which quickly gives, to even an untrained eye, knowledge of the nature and origin of amyloplasts.

Let us first say that all the oval-shaped elements visible in the cells are filled with starch and give the characteristic coloration by iodine. They are therefore indeed amyloplasts. The reader who will examine successively all these oval bodies and compare their shapes with those represented in figures 5, 6, 7, 8, of plate 14 will find them identical in all points; one will find there all the characteristics listed above and, in addition, certain very important particularities not noticed by **Guillermond** and which will be discussed later. This comparison will be even more suggestive if one will compare the shapes of the elements contained in the figures of plates 13 (fig. 1 to 6) and 14 (fig. 1, 2, 3, 4, 10, 11) of this book, as well as their position or location in the cells, with the shapes of the elements called amyloplasts that most of the plates (as well as figs. 4 to 11 of the text) in **Guilliermond's** thesis contain.

Let us first note that, in the figs. in plates 13 and 14, the nucleus is still in its place in the cells and this is because a part of the undislocated cytoplasmic network has remained adherent to its periphery and attaches it at several points to the external membrane. If we examine the cytoplasmic network around the nuclei in the six figures in plates 13 and 14, we note:

1. That each of the meshes or alveoli of this network is an amyloplast, often polyhedral in shape;

2. That at the periphery of the remaining network, the dislocation of the meshes sets free these amyloplasts to which a portion of the dislocated meshes remains adherent;

3. That these portions of meshes are precisely those which constitute the tails of the plastids or give to some of their edges (or to the entire circumference) the appearance of a thickening.

Other fragments of the cytoplasmic network also remain adherent to the external cell membrane and it is found that each mesh is an amyloplast which, in detaching itself, takes with it fragments of the network. There exists in the cell situated on the left of figure 2, plate 13, a point formally demonstrative on this subject; it comprises a group of 6 or 7 amyloplasts joined together by a fragment of the cytoplasmic network not destroyed and connected to the cell membrane by several dumbbells ***a, b, c***; of the upper dumbbell ***a***, whose corroded rod is in the process of breaking and perhaps already broken, there remain only two adherent projections, one to the amyloplast ***f,*** the other to the cell wall; the mucro ***k*** of the amyloplast ***l*** and the mucro ***m*** of the amyloplast ***n*** of the same figure 2 are of the same origin and the same nature; Two other projections or mucros of the cell wall, one ***d*** located a little above the corroded dumbbell ***a***, the other ***e,*** belonging to the neighboring cell, are also the residues of two dumbbells, one of the balls of which was articulated with the cell wall.

This shows by what mechanism and under what influence the amyloplasts gradually detach themselves, either individually or in groups, and explains the significance as well as the nature and origin of the mucros or tails that these so-called amyloplasts carry with them (tail ***p*** of amyloplast ***o***, Fig. 5).

Let us return to the group of amyloplasts ***f, g, h, i, j*;** between the amyloplasts ***f, g, h,*** we see, in thick black lines, the dumbbells of the cytoplasmic network that separate them; the amyloplasts ***i*** and ***j***, which can still be seen by a light shadow, are burst and in the process of complete dissolution.

It is visible that the dumbbell **i'** is in the process of corroding and breaking; when it is broken, the group of three alveoli ***f, g, h***, will become free and will constitute a group of three amyloplasts linked together by the residue of the cytoplasmic network carried with them.

This is how the groups of 2 or 3 (or more) amyloplasts are formed, which **Guilliermond** has depicted and whose drawing is reproduced in figure 6, plate 14; the group of three amyloplasts placed on the left of this figure 6 carries in itself the proof that the thickenings of the wall of these plastids, all situated on the left, are in reality a portion of the cytoplasmic network preserved on this side in the form of a continuous filament common to the three plastids; this is put beyond all dispute by the fact that the upper plastid, clearly isolated and separated from the group of the two lower ones by a free space, is nevertheless joined to them by this same filament, which, isolated and clearly visible in the free space separating the upper plastid from the other two, appears incontestably there as an element completely independent of the so-called plastids and as belonging to the cytoplasmic network which has resisted destruction.

It is thus confirmed by this fact and by **Guilliermond's** own drawings that plastids are not individualized formations, having a physiological significance, but are only abnormal elements, in the process of destruction and resulting from the necrosis of the cytoplasmic network and its dislocation under the influence of the action of the fixative and other liquids used for the inclusion of tissues and their staining.

In summary, this shows:

1. That the projections, mucros, or tails presented by the amyloplasts drawn by **Guilliermond** are the remains of the dumbbell organelles of the disorganized cytoplasmic network;

2. That the alleged thickenings shown at certain points on the periphery of the amyloplasts by **Guilliermond** are the rods of the dumbbell organelles of the cytoplasmic network carried along with them when it disintegrates.

These thickenings are the large black lines seen, for example, in Figure 2, Plate 13 of this book, between the amyloplasts ***f, g, h***; the reader who will examine the cytoplasmic network preserved around the nuclei of the cells in Figures 2 to 6 of Plate 13 will see various types of these thickenings, some of which, ***t***, Figure 3, are perfectly and totally preserved dumbbells, or only partially, ***z,*** Figure 3; ***y, z***, Figure 6; the latter ***z*** has two contiguous balls ***w***, which complete its identification as a dumbbell organelle, this one never articulating with others other than by its balls.

We will now find in the figures of our plate 14 new elements likely to establish absolute conviction regarding the nature and origin of amyloplasts as well as the nature of the expansions and thickenings depicted at the periphery of amyloplasts by **Guilliermond**.

We see in this plate:

1. In Figure 5, **Guilliermond's** conception of cellular organization; a conception in which the nucleus would be devoid of any connection with the cell wall, that is, floating in the cell, and the living matter of the cytoplasm would be constituted by formless elements, scattered without any connection between them in the cytoplasmic space; a drawing of the cell represented demonstrates the total destruction of the cytoplasmic organization; this demonstration is established, firstly, by the enormous vacuole which occupies half of the cytoplasmic space; secondly by the total absence of connections with the wall of the nucleus, which, no longer being maintained by these connections in the central region of the cell, has come to stick to the external membrane; thirdly by the formless debris scattered in the cell and which, quite obviously, come from the dislocation of the cytoplasmic network;

2. In figures 6, 7, 8, 9, the different forms and particularities that **Guilliermond** assigns to amyloplasts. These figures are photographs of figures 4 page 41 and 10 page 79, of his memoir (10);

3. At certain points, so-called amyloplasts whose perimeter is made up of dumbbells articulated between them by their clearly distinct balls, for example in plastid a figure 2 and in plastid ***b*** figure 4, which demonstrates that they are alveoli of the cytoplasmic network;

4. In figures 1, 2, 3, 4, 10, 11, plate 14 amyloplasts still included in the portions of the undestroyed cytoplasmic network, either around the nucleus or adhering to the cell wall, which proves that these plastids are not distinct, characterized elements, but only alveoli of the cytoplasmic network.

An example which gives this demonstration in an indisputable way is provided by figure 4 where we see a group of 4 amyloplasts ***b, c, d, e***, still linked to the cell wall by the unde- stroyed cytoplasmic network; the amyloplasts bet ***c,*** connected to each other by a thick tract, form an image identical to that which **Guilliermond** represented in his figure 8, plate 2, reproduced figure 5, plate 14 of this book, attached to the lower right part of the nucleus of the cell, thus showing that these so-called amyloplasts are in reality only alveoli of the dislocated cytoplasmic network. Amyloplast ***c*** is itself adherent to amyloplast ***d*** and this to amyloplast ***e,*** the latter two being still fixed to the cell wall. Let us also note that these amyloplasts have kept a polyhedral shape which identifies them as being alveoli of the cytoplasmic network.

Moreover, this identification is still established and formally confirmed by the dumbbells which surround these plastids, dumbbells of which some of the balls are clearly visible; four of these balls are articulated together at point ***f***; Their rods form a cross, the lower branch of which is formed by a dumbbell, one ball of which is articulated at point ***g*** with the cell membrane.

Fig. 10, Pl. 14, provides another demonstration of the nature and origin of the tails that **Guilliermond** depicted on the periphery of amyloplasts, such as, for example, the two tails of the amyloplast located in the lower right corner of Fig. 6, Plate 14 (**Guilliermond** drawing photographed). Cell ***k*** in Figure 10 has its cytoplasm destroyed in its central part, but the alveoli of the periphery, contiguous to the cell membrane, are preserved and still adherent to this membrane. One of them, alveolus ***h***, is identical to that of the lower right corner of Figure 6 drawn by **Guilliermond**; as it carries two tails, ***i*** and ***j*** which are rods of the dumbbells of the cytoplasmic network; this alveolus which, according to **Guilliermond** would be an amyloplast, is thus identified without possible dispute

by its position and its adhesion to the cell membrane. The same demonstration has already been given by the amyloplast ***o*** adhering to the cell wall and its tail ***p*** (fig. 5, pl. 13); the dumbbells ***a*** and ***i,*** of figure 2, plate 13, in the process of corrosion and rupture also show how these tails are formed and where they come from.

We also find in **Guilliermond's** drawings an equally clear demonstration of the meaning and origin of these tails. We see in figure 5 of the plate concerning the radicle of the castor bean seed and in the middle of the upper edge of part 6 of this fig. 5, (reproduced in fig. 1 of our plate 15) a compound starch grain equipped with an upward-pointing tail, which is constituted by a complete dumbbell provided with its two balls, one located inside the starch grain, the other at the end of the tail.

The reader who will examine the figures in plates 13 and 14 with a magnifying glass will find at many points the various forms of plastids drawn by **Guilliermond** and will easily identify them himself.

This identification of amyloplasts with the alveoli of the cytoplasmic network, as well as the demonstration of their origin as elements resulting from the dislocation of this network in the process of destruction, therefore leads us to this simple conclusion: amyloplasts do not exist; it is only through errors of observation and reasoning that **Guilliermond** concluded the existence of these bodies as individualized formations having a special physiological role, the formation of starch. These so-called isolated and free plastids depicted by **Guilliermond** are only debris in the process of total necrosis, freed by the dislocation of the cytoplasmic network altered by the fixative and by the liquids used for inclusion and for staining.

The precision of the demonstrations that lead to these conclusions, their multiplicity, place them beyond all dispute, as does the conclusion they lead to: the non-existence of plastids. These cannot therefore, for this peremptory reason of their non-existence, come from the evolution of small dilations of chondrioconts, as **Guilliermond** asserts.

Let us recall that it has already been demonstrated previously that chloroplasts do not come from the evolution of these small dilations and that their constitution is in formal opposition to such an origin.

* * *

It remains to be demonstrated that the presence of starch in the alveolus, a substance with which the so-called plastids appear to be stuffed, does not give it the character of an individualized plastid. Certain observations by **Guilliermond** provide this demonstration.

Using an iodine solution as a reagent, **Guilliermond** observed that the dilations he observed along the path of filamentous chondrioconts are colored by iodine in the same way as the elements he called amyloplasts. He relied on this fact to assert that amyloplasts result from the evolution of chondriocont dilations and that these are a young stage of amyloplasts.

A student of **Guilliermond**, **P. F. Milovidov** (50), confirmed these results either by the procedure indicated above, or by another method consisting of staining the preparations with **Hcidenhaim's** ferric hematoxylin, then with a 1% aqueous solution of methyl green. Here is how he expressed himself on this subject:

*It can now be considered established that a portion of chondriosomes, the so-called active chondriosomes, play a role in photosynthesis, by transforming into plastids and producing starch (**Lewitsky, Guilliermond**). The precise facts highlighted by **Guilliermond**, which demonstrate that starch, when it is not formed in chloroplasts, always originates within chondriosomes, are still often insufficiently known; and yet, **Guilliermond** has succeeded in providing irrefutable proof of this mode of starch formation, by detecting starch grains within chondriosomes...*

And he concludes:

Thus, it is possible, using the methods we have just indicated, to obtain a double stai - ning that allows us to differentiate the smallest starch grains within the chondriosomes, to follow the various stages of their formation, and to obtain stable preparations. These preparations clearly demonstrate the formation of these grains within the elements of the chondriome…

Milovidov provided drawings of the observed elements in his memoir. I have photographically reproduced these drawings in plate 15 (figs. 1 and 2), but without being able to preserve the double coloration. It will be seen in these drawings that the chondrioconts exhibiting dilations clearly appear as fragments of dumbbells. It is obvious that the varied and numerous forms of these elements prove that they are only stumps of normal elements of the cytoplasm that have been altered and dislocated.

Without wishing to discuss either these staining methods or the results obtained, I will only note that it is not surprising that the balls of the dumbbells give the coloration of starch since this body is formed in plant cells and these organelles constitute the entire cytoplasmic network, that is, of living matter of the cytoplasm.

But we will see that this fact in no way leads to the conclusion that the bodies called amyloplasts by **Guilliermond** really exist and come from the evolution of the dilations of the chondrioconts.

Indeed: if we refer to plate 13 of this work, we can notice that a certain number of these so-called plastids are formed by several large granulations. This is very visible, for example, in plastids ***q, r, s,*** in figure 6; these granulations are very large, colorless, and refrigerant; but a distinct shadow separates them and allows us to limit their perimeter. This establishes that the interior of the element is not uniform and is formed by swollen grains packed against each other.

If we consider, on the other hand, that the interior of the cytoplasmic network is normally occupied by a liquid that can circulate freely, and not by a thick or gelatinous substance that opposes all circulation; that, on the other hand, the alveoli do not contain granulations apart from the balls of the dumbbells that exist only at their points of articulation, which are the vertices of the angles of the polyhedra, we are led to conclude that the presence of these large refrigerant granulations in the so-called plastids is abnormal and results from the same actions that cause the dislocation of the cytoplasmic network.

Where do these refrigerant granulations come from? They can only have two origins: either they are dumbbell balls which, swollen by the action of the fixative, come to fill the interior of the alveoli, or they are small starch granules which, free in the cytoplasmic liquid, swell under the same influence. We can also imagine that the starch which would be in colloidal solution in the cytoplasmic liquid is precipitated by the fixative in the form of balls, rounded lumps which fill the alveolus.

But a careful examination of the details of the figures in plates 13 and 14 shows that it is the balls of the dumbbells that swell and fill the interior of the alveoli. It is also known that, under the influence of fixatives, the granules or granular mitochondria of the cytoplasm swell and eventually burst and disappear.

Now, it is also known that these granular chondriosomes are seen at the apexes of the angles of the alveoli (**Renaud, Policard, Gilbert**, and **Jomier**), and all the observations in this volume show that if these granular chondriosomes are located in this place, it is because they are the balls of the dumbbell organelles and because these, which form the solid skeleton of the alveoli, that is, of the cytoplasmic network, never assemble together except by their balls.

These observations therefore show that, under the influence of the fixative liquids, the dumbbells, balls, and rods swell considerably. If the swelling of the balls becomes too considerable, they end up filling the alveoli, then bursting them, which causes the dis-

location of the cytoplasmic network and the separation of a certain number of alveoli, either isolated (simple starch grains) or grouped in twos, threes, or more (compound starch grains).

This dislocation of the cytoplasmic network occurs all the more easily because the rods of the dumbbells, themselves attacked by the fixative, corrode at certain points and break or dissolve.

The fact that, in the figures on plates 13 and 14, some dumbbells remain fairly well preserved and show their balls very slightly inflated is in no way in contradiction with the explanations I have just given; in fact, the alteration of the dumbbells by the fixative occurs in a very irregular way, the balls of some being barely increased in volume while those of certain others are ready to burst.

The so-called plastids are therefore indeed constituted by the swollen balls of the dumb bells, balls that **Guilliermond** calls dilations of the chondrioconts; but this is not a formative evolution: it is an involution by alteration and an evolution towards the death of the elements.

These observations therefore do not detract from the rigor of the conclusions formulated previously. They only show the causes of the destruction and dislocation of the cytoplasm; they explain the causes of the separation of the so-called plastids from the cytoplasm and the nature or origin of their contents. They show, moreover, that it is the cytoplasmic network itself, that is to say, the totality of the dumbbells that constitute it, which elaborates the starch.

With this demonstration of the non-existence of amyloplasts, we conclude the series of studies aimed at verifying the accuracy of the currently accepted notions about mitochondria or chondriosomes, which have become dogmas.

These notions are false: mitochondria do not exist. What has been described under this name are the shapeless debris of the cytoplasmic network and the dumbbells that constitute it, altered and dislocated by fixing reagents.

Contrary to these notions, there is only one constructive organelle for all living beings, animals and plants, from the top to the bottom of the scale of beings. This universal constructive organelle has only one form, the dumbbell form, and also a universal mode of articulation, by its balls, to form cellular networks and filamentous elements.

From these facts we can therefore conclude that the false notion of mitochondria results from gross errors of observation and that it is one of the grossest errors that biology has ever known.

CHAPTER II

STRUCTURE OF THE CYTOPLASM OF ANIMAL TISSUE CELLS

We will successively study the cytoplasm of animal tissue cells:

I. In liver cells;

II. In the cells of the adrenal capsules;

III. In the cells of the renal epithelium;

IV. In the cells of the spinal ganglia;

V. Finally, we will study the constitution of myelinated nerve fibers.

I. CONSTITUTION OF THE CYTOPLASM OF LIVER CELLS

On page 27, you will find a retrospective account of the first precise knowledge of the liver cell, due to **Küpfer, Heidenhain, Flemming**, etc. I will limit myself here to referring the reader to **Noël Fiessinger's** memoir on the liver cell (20), where he will find, along with a historical account and very detailed explanations, a very complete bibliography.

Renault (1898) was the first to observe in the liver cell "*protoplasmic trabeculae radiating around the nucleus and intercepting by their contour meshes at the nodal points of which are the protein granules*."

In 1906, **Gilbert** and **Jomier** (24) confirmed this fact and observed that: *The protoplasm of the liver cell is composed of an areolar network whose meshes extend regularly over the entire surface of the cell... The nodal points are occupied by small granulations...* etc.

Then, in 1910, **Policard** (60) observed a protoplasmic framework, the spongioplasma, in the trabeculae of which there is a device of absolutely clear chondriosomes, in the form of short choncrioconts, arranged one after the other, in a row, in each trabeculae and he noted that the "*whole of the chondriome reproduces the design of the spongioplasma*".

Subsequently, studies on the chondriome introduced into science the inexact notions which were previously exposed in this volume and which stopped all progress. Indeed, we are now seeing the appearance of publications in which the constitution of the cytoplasm is completely unknown and where the scattered and shapeless debris of the destroyed cytoplasmic network are presented as normal elements of the cytoplasm.

We know that **Renaut** and then **Regaud** (67) attributed to the elements of the chondriome, but without any objective reason, the property of being able to exercise selection in the chemical transformations of which they would be the agents (eclectosomes).

Another notion gradually developed. The elements of the chondriome would be mobilized during the secretory period of the cell. They would not occupy the same place from the beginning to the end of the secretory act and, moreover, they would change shape. For example, a chondriocont would transform into a granular chondriosome during the secretory act.

Policard (61) was the first to put forward this concept of chondriosome mobilization. He distinguished two regions in the chondriome: a juxtanuclear part and a peripheral part; he indicated that the first is most often composed of short, stubby rods applied tangentially against the nuclear membrane. Let us first point out that this arrangement of chondriosomes indicated by **Policard** around the nucleus only exists in cells whose cytoplasm has been destroyed by the fixative.

When the cytoplasm is intact or fairly well preserved, the rods (or choncriocontes) of the cytoplasm are always arranged perpendicular to the surface of the nuclear membrane or nearly so and very often assume a radiating arrangement around the nucleus, each rod being located in the extension of the latter's radius.

The reader will find in plates 17, 18, 19, examples of these arrangements which are moreover a general rule for any animal or plant cell (see plates 3, 5, 7).

According to **Policard**, the grouping of chondriosomes around the nucleus is a general fact which, according to him, should lead one to suspect the existence of intimate functional relationships between the nucleus and the chondriome. Let us recall that it has been previously demonstrated that the perinuclear dumbbells, one of whose balls is pressed against the nuclear membrane, have among their functions, as does the whole cytoplasm, that of maintaining the nucleus in a fixed position in the middle of the cell. Here is **Policard's** conception of the chondriome of the hepatic cell:

It is interesting to ask what are the relationships that unite these two parts of the chondriome, juxtanuclear part and peripheral part. We believe that one and the other, far from being independent, represent only two different topographical expressions of the same cellular apparatus. All the facts that we have been able to observe lead to this conception that the chondiosomes pass successively from the vicinity of the nucleus to the periphery of the cell and from the periphery to the nucleus. There is a true circulation of the chondriosomes. The protoplasm of the cell is not immobile or fixed: it moves and carries the chondriosomes with it in its slow journey.

In a long memoir, **Robert Noël** (55) developed this concept and described the secretory cycle of chondriosomes. We will study this memoir later. But, from now on, I indicate here that it will be demonstrated:

1. That the protoplasm of the liver cell and of any cell in general is immobile;

2. That the protoplasm being the cytoplasmic network, the elements that constitute it, the dumbbell organelles, are definitively immobilized there; the chondriosomes being only the constituent parts of the dumbbells, it is therefore impossible for there to be a circulation of chondriosomes, as **Policard** conceived;

3. That the secretory cycle of chondriosomes therefore cannot exist and does not exist, the cytoplasmic network being the supporting framework of the cell, and the mobilization of the dumbbell organelles signifying the destruction of this framework, that is to say, the destruction of the cell.

* * *

Let us now turn to the presentation of the facts that provide the demonstration of the actual constitution of the cytoplasm of the hepatic cell. Let us first examine the technique used to achieve this.

The very numerous tests that I have carried out with the various used fixative fluids have shown that hepatic tissue and renal tissue are extremely sensitive to the altering action of these fluids, and the latter even more so than the former. Almost consistently, they destroy the cytoplasmic organization of the cell. After their action, the existence in the cytoplasm is only observed to be debris of the altered, then dislocated, cytoplasmic network.

Special fixatives, called mitochondrial fixatives, that is, formalin-containing potassium dichromate and fixatives based on osmic and chromic acid, as well as formalin used alone or in **Locke's** fluid, all exert this altering action.

The assertion that in some of them, bichromate-formaldehyde preserves the cytoplasm *rigorously, as well as possible***,** is inaccurate; the tests that I have carried out have shown me that the postchromization process whose effect would be to improve this preservation in no way restricts the altering action of bichromate-formaldehyde; proof of this fact is provided by the drawings of cellular structure of **Maurice Parat** showing a complete destruction of the cytoplasmic organization in the cells treated by these methods and by a special postchromization process; an example of this is provided by figure 5 of plate IV of this author's memoir (23) representing a pancreatic cell of the Axolotl. Figure 2 of plate 21 of this volume is the photographic representation of this figure. We notice there long flexuous chondrioconts which are debris of the cytoplasmic network.

The upper region of this cell presents numerous small vacuoles whose arrangement leads us to wonder if, in this region, the cytoplasmic network was not partially preserved, but poorly colored; but since this figure is a drawing and, moreover, made by a draftsman and not by **Maurice Parat** himself, the most careful examination does not allow us to appreciate the state of preservation of the cytoplasm of this region of the cell because it lacks details that could allow it to be appreciated and that a good photograph would probably have contained. This is a typical example of the serious drawbacks of drawings. If this cell had been photographed correctly, one could easily appreciate the state of the cytoplasm at all its points.

Microphotography constitutes an absolutely perfect recording method for microscopic anatomy; a work that does not use this process suggests that its author's preparations were defective, unpresentable. Indeed, how can one explain that an author uses the process of drawing, which is expensive if he has it done by a specialist, or long, if he does it himself and which is moreover fatally inaccurate and incomplete, since it cannot include in the drawing what has escaped the observer, while microphotography allows the image to be fixed in five to ten minutes with absolute accuracy and in its smallest details *which will remain visible on the photographic prints on paper.*

This process, which only requires the expense of a photographic plate, is very rapid, and extremely simple and easy to execute, means that it must be concluded that if an author does not use it, it is because he does not want to or cannot show his preparations and prefers the method of semi-schematic drawing which can be arranged as he wishes by combining indications collected at different points. This is a process which inevitably leads to major errors.

In seeking to determine the various conditions under which liver tissue is least altered by preparatory procedures, I was led to observe that the iron alum and ammonia solution and the hematoxylin solution used for staining sections, themselves altered the cytoplasm. Indeed, by staining rat liver sections fixed with formalin with silver blue, the preservation of the fine cytoplasmic network visible in Figures 4, 5, and 6 of Plate 21 was observed. By treating other sections of the same series with a 0.5 or 1% iron alum solution heated to approximately 45°C for 3 to 10 minutes, then for 1 to 10 minutes with a 1% iron hematoxylin solution heated to the same temperature, the cytoplasmic network was observed to have been almost completely destroyed.

Alcohol itself produces notable alterations, notably a retraction of the cells causing cracks in their interior.

The sensitivity of the cytoplasmic organization of hepatic cells (and of all cells in general) to altering actions is not the same in the various species. It is greater for the mouse liver than for that of the rat and greater in the rat than in the guinea pig, where the cytoplasmic network can be stained without destroying it, as shown in plates 17, 18, and 19. It should be noted here that the appearance of the cytoplasmic network varies with the degree of alteration due to the fixative and notably with the degree of swelling of the balls of the dumbbells. After multiple tests, the fixative liquids which seemed to me to alter the liver cells the least are:

1. A solution of neutral formalin of 6 to 10% **(1)** in distilled water or containing 8 to 10 p. 1,000 of Nacl;

2. A **Locke's** solution containing 5 to 10% of neutral formalin modified as follows:

Sodium Chloride	8 grams
— Potassium	0.40 g
— Calcium	0.20 g
Sodium Bicarbonate	0.20 g
40% Neutral Formalin	125 to 250 cc
Distilled Water P.	S. per 1 liter

In most cases the solution contained 250 cc. of 40% formalin, that is, it was a quarter-strength formalin solution;

3. An aqueous solution containing 7 to 8% formalin and 2 g. % of mercury bichloride.

Helly's fluid gave me appreciable results in several cases, and poor results in others, as Figures 1 to 8 of Plate 20 prove.

I have had defective results using fixatives containing chromic acid. However, in one case, I obtained an appreciable result using a 10% formalin solution with 1% chromic acid added, injected slowly into the liver through the portal vein immediately after death. Plates 17 and 18 relate to this case. It is discussed later. Subsequent tests carried out under exactly the same conditions gave only negative results. Whichever of these fixing liquids is used, consistent results are never obtained. Most often, the cytoplasmic network is destroyed or very altered; in other cases, it is quite well preserved without it being possible to assess the reason; this inconsistency of results is, moreover, well known to histologists.

(1) *The strength of formalin solutions is always indicated, in this work, in absolute value of formalin (formic aldehyde) and not in proportions of the commercial formalin solution.*

But if the alteration of the cytoplasm is not too complete, it is not an absolute obstacle to its study, because portions of the cytoplasmic network often remain intact enough to allow their composition to be determined.

* * *

Let us now examine the photographic documents contained in plates 17 to 20. It must be remembered when examining them that, despite the preservation of the cytoplasmic network, it is always more or less altered. The alteration begins with the swelling of the balls and rods of the dumbbells, whose ability to fix the coloring materials (in particular hematoxylin) gradually diminishes, then their substance dissolves until it disappears completely. It will be noted in the photographs that the altering action is not equal for all the elements; certain dumbbells and even certain regions of the cytoplasm are already destroyed while in other points of the same cell, the elements are fairly well preserved; certain dumbbells even appear to be free from any alteration. It is from this inequality in the altering action of the fixative that the formation of the vacuoles of the cytoplasm results.

The figures in plates 17 and 18 are photographs of hepatic cells from the guinea pig liver fixed by passing a 10% formalin solution containing 1% chromic acid into the organ through the portal vein: the liquid circulates slowly for 30 minutes under the pressure of a 15-centimeter-high column of water. After this time, slices 1 to 2 millimeters thick are cut from the liver, which is already noticeably hard, and then immersed for another 24 hours in the same solution, then for 24 hours or several days in a 10% formalin solution; finally, they are subjected to the series of operations for paraffin embedding.

The sections are then immersed for 2 to 10 minutes, most often 2 to 3 minutes, in a 0.5 or 1% iron alum and ammonia solution, heated to between 40 and 45°C. Then, after a single very quick wash, they are immersed for 2 to 10 minutes in a hematoxylin solution also heated to between 40 and 45°C and containing: 1% hematoxylin, 10% glycerin,10% 90° alcohol, 80% distilled water.

After washing, the preparations are either dried slowly or passed through absolute alcohol, then toluene, and then covered with cedar oil without letting them dry, or mounted in balsam.

The most advantageous method is to let the sections dry slowly and preserve them this way, which allows them to be re-stained later if necessary. I have been satisfied, in many circumstances, that the appearance of the cytoplasmic organization is exactly the same in these three cases and that the retraction that desiccation can cause, in certain cases, is not greater than that caused by alcohol and that, moreover, it does not change anything in the cytoplasmic organization.

We will see later that, in certain cases (rat liver), air drying the preparation, after staining with an aqueous solution of silver blue (Azur II) for example, is the only process that allows the cytoplasmic network to be properly examined and photographed, because staining with hematoxylin often alters it; since silver blue staining is quickly removed by alcohol, it is therefore necessary, in order to be able to examine the preparation under cedar oil, to air dry it first. After treatment with absolute alcohol, then with toluene, such a preparation still retains enough dye for the cytoplasmic network to be visible, which allows us to ensure that the retraction of the cells is no less than after simple air drying.

I point out, to conclude these technical indications, that differentiation has never been practiced after staining with hematoxylin, except in exceptional cases (study of the nucleus) and to reduce overly violent staining; the staining process has had the sole aim, in my observations, of making all the elements represented visible, as well as their particular details. Differentiation practiced until obtaining an electivity that is highly questionable, has the effect of eliminating the visibility of certain elements of the utmost importance. This, combined with the altering action of the fixing liquids, makes it impossible or even difficult to observe a cytoplasmic organization.

The best result has been obtained by carrying out the staining with care and stopping it when it seems sufficient.

* * *

Here are the facts made apparent by the photographs in plates 17 and 18.

The existence of the cytoplasmic dumbbell network is observed in all cells, but with differently accentuated alterations, often very profound. Despite these alterations, these photographs establish:

1. The constant existence of the cytoplasmic network in all hepatic cells;

2. The constancy of the shape and characteristics of this network;

3. Its constitution by dumbbell-shaped organelles articulated together by their balls; even in cells in which these balls are no longer visible, the assembly of rods (which are the chondrioconts) is preserved and gives the network its characteristic alveolar appearance;

4. The articulation of the dumbbells by their balls, on the one hand with the cell membrane and, on the other hand, with the nuclear membrane, generally has a radiating arrangement around the nucleus;

5. The role of the network in maintaining the cell's shape and in maintaining the nucleus in an immutable position in the middle of the cell.

In Plate 17 we see:

In Figure 1, two liver cells at the edge of a torn section, the lower one showing a portion of the cytoplasmic network with two rows of dumbbells (magnification 1.100).

In Figures 2 and 3 (magnification 1.650), the same liver cell photographed at two different focuses and having a network with 2 or 3 rows of dumbbells, some of which clearly show one of their two balls, unstained and which, under the microscope, appeared as a shiny, highly refractile hyaline globule.

In Figures 4 (magnification 1.100) and 5 (magnification 750), trabeculae of liver cells with their networks.

In Figure 6 (magnification 1.150) two cells, one of which, the larger one, shows an altered cytoplasmic network with several rows of dumbbells and in the process of dislocation and the other, the smaller one, a crown of a single row of radiating dumbbells occupying two-thirds of the circumference of the nucleus.

In Figures 7, 8, and 9 (magnification 1.100) cells with several rows of dumbbells. Figs. 8 and 9 are two photographs of the same cell at two different focuses showing a cytoplasmic network with 7 or 8 rows of dumbbells at some points and destroyed (vacuolated) at other points.

The photographs of plate 18 examined under a magnifying glass will show exactly the same facts, the same alterations of the cytoplasmic network and its constituent dumbbells. Figures 1, 4, 6, and 7 show the radiant arrangement of the perinuclear dumbbells.

Plate 19, which also concerns the guinea pig liver, this time shows hepatic cells stained with ferric hematoxylin whose cytoplasmic network is almost completely intact. These are sections of a liver dotted with the numerous small islands of caseous matter that characterize rodent pseudotuberculosis. This liver, taken immediately after the animal's death, had been fixed by immersion for 42 hours in a formalin solution to which 0 cc of 5% acetic acid had been added.

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Figures 1, 2 (magnification 750) and 5 (magnification 1100) show cells with a cytoplasmic network that appears almost intact, but whose constituent dumbbells have nevertheless undergone a notable swelling. At the points where their balls are clearly visible, notably in Figure 22, they are seen to be greatly swollen; some points, notably in Figure 1, are in the process of vacuolation and show dumbbell rods in the process of dissolution. But this latter alteration cannot be attributed solely to the action of the fixative because the large vacuoles visible at certain points result from the destruction of cells or groups of cells by the same cause or the same agent which causes pseudo-tuberculosis. Figures 3 and 4 (750 × 1000) show several of these areas of destruction, one (Fig.

3) having preserved the cytoplasmic network in the process of destruction and two others (Fig. 4) containing only a few scattered elements of the completely destroyed network.

Plate 20 was prepared to show the varied forms of the elements observed in cells whose cytoplasm has been completely destroyed. Figures 1 to 8 are photographs of sections of a guinea-pig liver fixed for 48 hours in **Helly's** fluid; Figures 9 to 13 of a guinea-pig liver fixed for 4 days in a 10% formalin solution. In all the figures, the cytoplasmic organization is completely destroyed, but the alterations are more profound in the case of **Helly's** fluid than in the case of formalin alone.

In Figures 1 to 8 (**Helly**), all that remains in the cell are filaments and other shapeless elements that have largely lost their ability to stain are scattered or piled up haphazardly in the cytoplasmic space. The small mass of 3 to 4 millimeters in diameter in Figure 4 (lower third) from which 3 slender filaments branch off is what remains of the articulation of 4 or 5 dumbbells; this figure shows both the origin of such small masses (group of dumbbell balls articulated together) and that of the dumbbell rod filaments.

In Figs. 9 to 13 (10% formalin), the cytoplasmic network is also completely destroyed, but the alterations are a little less profound, because the cell nucleus has retained some tracts that still connect it to the cell wall. In Figs. 9 and 10, we can even distinguish fine tracts that are the last vestiges of the disappearing cytoplasmic network. In all these figures, the residual elements of the dislocated network show by their shapes, however varied they may be, that they are all fragments of dumbbells, either isolated or still united together.

* * *

The secretory cycle of the chondriome, according to Policard.

We will now move on to the study of **Robert Noël's** thesis (21) cited at the beginning of this chapter. It relates to the normal constitution of the cytoplasm of mouse liver cells and the physiological role of the elements that constitute it.

The elements that **Robert Noël** describes in this thesis under the name of mitochondria as normal morphological elements of the mouse liver chondriome are only the scattered, shapeless debris of the cytoplasmic network dislocated and largely destroyed by the preparation procedures (fixatives, liquids used for inclusion, staining operations). The liver cells examined by **Robert Noël** therefore had their cytoplasmic organization completely destroyed, and this destruction is attested to by the drawings contained in this thesis. One of these figures (fig. 2, p. 30 of this thesis) shows a liver cell with the

elements of the cytoplasm that **Robert Noël** considers normal and with the arrangement they occupy therein.

The other (pl. 7 of the memoir) is a schematic figure showing where, according to the author's conception, the progressive transformation of the filamentous elements, the chondrioconts, into granules during the secretory act must take place.

These two figures are reproduced in plate 21 of this book (figs. 1 and 3); the other figures in this plate are photographs of mouse liver cells (fig. 4) and rat liver cells in which the normal cytoplasmic organization, that is, the cytoplasmic network and the dumbbell organelles that form it, are preserved. Comparison of figures 1 and 3 with the other three puts beyond any discussion the fact that, in the cells examined by **Robert Roël**, the cytoplasmic organization was destroyed and that the elements he considers normal are only scattered and shapeless debris of the destroyed cytoplasmic network.

In the cells in which he depicted these various elements, we find, as in the plant cells depicted by **Guilliermond,** no indication of a cytoplasmic organization; we see no link between the nucleus and the cell wall, a fact which is the infallible criterion for the destruction of the cytoplasmic organization.

Robert Noël attributed the following forms to the elements of the chondriome that he considers normal: "*Granular mitochondria, chondrioconts, filamentous elements with swollen ends, water-drop figures, grains with tails, granulations which are of two kinds: solid grains and grains with shells*."

This enumeration, as well as **Robert Noël's** drawings, show that these elements are the same as those observed by **Guilliermond** in the plant cell, the nature and origin of which have been studied in the various preceding chapters. I recall that it has been demonstrated by multiple proofs that these elements are indisputably the scattered and shapeless debris of the dislocated and largely destroyed cytoplasmic network, and that they are destined to disappear, that is, to undergo necrosis and not evolution; their varied form results from the chance points of rupture or disintegration of the dumbbells that constitute the network.

Robert Noël considers all these varied forms as stages in the evolution of mitochondria during the cell's secretory work, with the chondrioconts and filamentous elements gradually transforming into granules. This progressive transformation is shown schematically in plate 7 of his memoir, reproduced in figure 3 of plate 21 of this volume.

This is how **Robert Noël** expresses himself (p. 31 of this memoir), on the subject of the various elements that I have listed above:

Is there a link between these various elements? From the examination of our preparations, it appears that, apart from the ring-shaped mitochondria, all the figures just discussed are only the morphological expression of the successive transformations undergone by the same element, the chondriocont; filamentous and very slender at functional rest, this element becomes larger, more visible at the moment of its entry into operation; its diameter increases and passes from 0µ2 to 0µ6, at the same time as its length appears to decrease. Then little by little we see one of its ends swell, take on a pyriform appearance; the whole presents the appearance of a club which grows, becomes rounded and appears to absorb the substance of the initial chondriocont; the latter is now represented only by a tail which becomes shorter and shorter. This is the stage of the tailed grain, immediately preceding the definitive granulation, the end of the ascending evolution and whose size is relatively considerable. Chondriosomal filament, club and water drop shapes, tailed grains, granulations or plastids, these are the beginning, the intermediate forms and the outcome of what could be called the ascending phase of the secretory cycle of the chondriome. This ascending phase is followed by a regeneration of the elaborating chondriome, as we will see later.

Let us first note that what **Robert Noël** thought to be the secretory cycle of the chondrioma resulted from reasoning and not from direct observation. The various forms he observed are indeed different stages of a transformation, but they are the stages of the progressive alteration of the cytoplasmic network by the fixative and not those of a secretory cycle of the chondrioma; this alteration is manifested by the progressive swelling of the elements, then by their disappearance, either after bursting or by complete dissolution in the liquid medium; they first gradually lose their ability to color, then when they have become hyaline and barely visible, they disappear. These stages of the alteration of the cytoplasmic network have already been described in the previous chapters.
On the other hand, there is an absolute material impossibility of the existence of a secretory cycle of the chondrioma characterized by the changes in form indicated by**Robert Noël** and this for the following reasons:

1. In reality, the chondriome does not exist, because what has been taken for mitochondria are the shapeless debris of the cytoplastic network destroyed and dislocated by the harmful action of fixing liquids. Therefore, there cannot be a secretory cycle of a chondriome that does not itself exist;

2. There exists in the cell only one form of living figured elements, the dumbbell, a fundamental organelle that constitutes the cytoplasmic organization and the nuclear organization and which, by articulating with others by its balls, a process that is uniform and exclusive, constitutes the cytoplasmic network and the nuclear network.

These networks constitute the living and active matter of the cell; they have, among their functitions, that, for the cytoplasm, of maintaining the nucleus in a fixed and immutable position at the center of the cell by connecting it on all sides to the cell

membrane, and that for the nucleus to maintain the stability of its shape and volume as well as that of the position of the nucleolus; they are the supporting framework of these two parts of the cell;

3. The cytoplasmic network of dumbbells is a stable organization, definitive for the entire duration of the life of the cell. The elementary organelles that constitute it are definitively fixed and immobile and there can be no question for them of a change of shape during a secretory act, because this change of shape would imply the disorganization of the network, which would be equivalent to a destruction of the cell, whose nucleus would become floating. There cannot therefore exist in the cytoplasm a secretory cycle accompanied by a change of shape of the elements of the kind that **Robert Noël** indicated;

4. *Regardless of when the liver tissue is removed, whether during the secretory period or outside of digestion, the cytoplasmic network always presents the same constitution and the elements that constitute it appear unchanged;* the different forms of mitochondria that **Robert Noël** has listed do not exist in the liver cell in one case any more than in the other; the only difference that the cell presents during the state of digestion is the appearance of corpuscles of non-living matter resulting from the transformation of food and, perhaps, the accumulation in the cytoplasmic liquid of soluble substances that the fixatives and other liquids used are likely to precipitate in the form of granules.

This therefore puts beyond doubt and any dispute the non-existence of the secretory cycle described by **Robert Noël**, a cycle that would be characterized by a progressive change in the shape of the mitochondria and which would be followed by a regeneration of this form. Let us repeat, the forms of mitochondria described by **Robert Noël** are not normal elements and are only the shapeless residues of the destroyed cytoplasmic network.

The secretory act is obviously carried out by the entire living substance of the cell represented by the cytoplasmic and nuclear networks and perhaps, to an extent unknown to us, by the fine mobile granules (of the size of 0 μ2 to 0 μ8 on average) that exist in the cytoplasmic space and move there with Brownian motion. These granules come from the blood and are the micrococcal elements of the colibacillus that have crossed the cell membrane with the blood plasma and exit to pass into the bile and the lymphatics.

* * *

Robert Noël himself criticized his own observations. He wanted to prove: "*that the figures he established are indeed real and are not artifices*" (p. 32 of the memoir).

To this end, he indicates that, since his preparations were fixed and stained using **Regaud's** method, "*he is justified in thinking that filamentous chondrioconts exist in vivo in the state shown to us by bichromate-formaldehyde fixation, since the results obtained by **Regaud's** method are strictly identical to those given by properly conducted vital observation*."

These two postulates are inaccurate. It has been demonstrated in the preceding chapters that the bichromate-formaldehyde fixation used by **Guilliermond** for plant cells completely destroys the cytoplasmic organization. For animal tissue cells, numerous tests, particularly on the liver cell, have provided me with the same demonstration of the destruction of the cytoplasmic organization by this same fixative.

As for the assertion that the results given by bichromate-formaldehyde are rigorously identical to those given by vital observation, it is purely gratuitous and has no objective basis. I have already discussed this point on page 33 of this book, on the occasion of the same assertion formulated by **Guilliermond**. Vital observation does not allow us to see the details of cytoplasmic organization; those who assert the contrary have only one way to prove it, which is to produce clear photographs that show these details.

Those who have attempted this, notably **Lewitsky** (41) to prove an origin of chloroplasts that was inaccurate, have produced photographs from which no conclusion can be drawn because they show nothing precise and because, moreover, it has not been established that the traces of filaments that we notice there are not simply foreign

bodies deposited on the epidermis, (bacteria, filaments, unicellular algae) which one observes most often on the leaves of *Elodea Canadensis*, the subject of these photographs; in any case, one does not observe, in these photographs, the slightest trace of cytoplasmic organization: the reader will be able to see this by examining one of them reproduced in figure 2, plate 12 of this volume.

In summary, it is false that dichromate-formaldehyde preserves the cytoplasmic organization; it destroys it. And to say that the cell treated with this fixative remains strictly identical to a living cell is not exact, since, through in vivo examination, the cytoplasmic organization is not seen. **Guilliermond** provided the most formal proof of this by affirming that he had verified his conclusions by examining the living cell; as all cytoplasmic organization was invisible (because destroyed) in the cells treated with his fixatives that he observed, he could not see it any more in the living cell since the vital observation

that he carried out could not show it to him, and the fact that he did not see it constitutes formal proof that vital observation does not allow us to see it.

Robert Noël adds, following these two erroneous postulates:

It is regrettable that we were unable to observe in the same way, in vivo, the series of intermediaries that separate the filament from the granulation. But, in the absence of this decisive argument, we believe that the following considerations are likely to lead to conviction.

b) *The images observed are not artifices. If the grains, the club, dumbbell, and water drop shapes are preparation artifices, they can only be due to two causes: fixation or autolysis.*

Influence of fixation. - Regarding fixation, we will first note that, reasoning by analogy, it is not clear why the same fixative acting on the same cell would leave certain elements intact: the filamentous chondrioconts, and alter others, the transition forms; both are, in fact, in strictly identical conditions.

It is also established that alterations dependent on fixatives consist essentially of coagulations of the protoplasmic gel. These coagulations, due to osmosis phenomena, must vary in intensity according to the osmotic power of the fixative used. However, we obtain images identical to those given by ***Regaud's*** *liquid after using* ***Mèves's*** *chromo-osmic solution.*

These two liquids, absolutely different from an osmotic point of view, should produce deformations of different appearance on the chondriome. This is not the case. The images, in both cases, are absolutely superimposable, provided, of course, that on the preparations fixed by the osmiochromic mixture we only take into account the superficial part of the sections, the only one that is fixed in due time, before any autolytic phenomenon.

Robert Noël says that we do not see why the same fixative, acting on the same cell, would leave certain elements intact, the filamentous chondrioconts, and alter others, the passage forms. He could not see this since he admitted that the filamentous chondrioconts are intact elements while they are the residues, already profoundly altered, of the destroyed cytoplasmic network. This fact alone removes all value from the argument. On the other hand, the elements of the cell membrane and the nucleus alter less quickly than those of the cytoplasm and, among these, some that alter more quickly than others are the starting point for the formation of vacuoles.

Robert Noël *admits that alterations dependent on fixatives consist essentially of coagulations of the protoplasmic gel*; the nature of these alterations is actually quite different. They consist of a progressive and fairly rapid attack on the substance of the dumbbells that constitute the cytoplasmic network, combined with a progressive swelling of their

granules and their rod. This attack generally results in the dislocation of the cytoplasmic network and, at the very least, the deformation of the scattered elements that remain.

Experience shows that these alterations occur with all fixative liquids and that the only way to limit them, if not avoid them altogether, is to study at length the action of the fixative as a function of time, its concentration, and the respective action of each of the elements that constitute it. Thus, through patient and lengthy studies, we arrive at fixations that, while not perfect, nevertheless allow us to observe the normal structure of the cell and the normal shape of its constituent elements.

Moreover, to bring about a complete and definitive conviction on this question, it is sufficient to demonstrate what the real constitution of the hepatic cell is, which, at the same time, provides an indisputable demonstration of the alterations brought about by formalin dichromate in the cells examined by **Robert Noël**. The photographs of the hepatic cells of the mouse and the rat (figs. 4, 5, 6, 7 of plate 21) provide the demonstration of the existence and the constitution of their cytoplasmic network.

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The mouse liver cell drawn by **Robert Noël** and reproduced in figure 1 of plate 21 forms such a striking contrast with the photographic images of the cytoplasmic network of liver cells in Figures 4, 5, 6, and 7 of this same plate that the demonstration of the destruction of the cytoplasmic network in Figure 1 and the nature and origin of the scattered and necrotic elements it contains are established beyond dispute.

Figure 4 is a photograph of a section of the mouse liver; Figures 5, 6, and 7 are photographs of a section of the rat liver. The cytoplasmic network is stained with silver blue in both cases; it is very fragile and alterable in both cases, and it is when stained with silver blue that it undergoes the slightest alteration. The network is more altered by the fixative in Figure 4 (mouse liver) than in Figures 5, 6, and 7 (rat liver).

Conclusions

In summary, we must conclude:

1. That the chondriome of the liver cell does not exist and that the free and unrelated elements of various shapes that have been described under this name are the shapeless debris of the cytoplasmic network altered and dislocated by the fixative or other liquids used for inclusion and staining;

2. That there exists in the liver cell only one form of figured element, the elementary organelle dumbbell which, by its articulation with others, constitutes the cytoplasmic network articulated on the one hand to the periphery of the nucleus and, on the other

hand, to the cell membrane, thus achieving the fixation of the nucleus to the center of the cell and the maintenance of its shape;

3. That the dumbbell organelles are constituted by a rod (which is the element called a chondriocont) bearing a ball or granular chondriosome at each end. In the intact cell there never exists an isolated chondriocont, nor free granular chondriosomes; there exist only dumbbells and these are always articulated together to form the cytoplasmic network; they only ever become free in cells in the process of destruction;

4. That because of this formation of the cytoplasmic network by the dumbbell, it is completely and definitively immobile and cannot change either shape or position;

5. That, in summary, there exists in the hepatic cell neither a secretory cycle nor a chondriome. The living matter of the cytoplasm (or protoplasm of the cell) is entirely constituted by the cytoplasmic network and its immobile dumbbell organelles;

6. That there does not exist, either in the hepatic cell or in any other cell, a protoplasmic or spongioplasmic network distinct from the elements called mitochondria; there is only one network formed by the dumbbell organelles articulated between them, the cytoplasmic network.

* * *

CONSTITUTION OF THE CYTOPLASM OF THE CELLS OF THE ADRENAL CAPSULES

It has long been known that the cytoplasm of the adrenal capsules has a reticulated appearance. But the constitution of the reticulum, the nature of the filaments that form its meshes, and their relationships with the granular chondriosomes contained in the cell are not currently determined; the constitution of the reticulum by a single elementary organelle, the dumbbell, is not known.

We will determine these different points by studying the constitution of the cytoplasm of the cells of the adrenal capsules of the guinea pig, the rabbit, and the cat.

The technique followed for this study is the same as that used for the study of liver cells. Thin slices of the organ were fixed for 3 or 4 days with a modified **Locke's** solution containing 10 to 12% formalin or with a 10% formalin solution with 1% acetic acid added in one case and 0.5% in another.

Staining was done with ferric hematoxylin without any differentiation and followed by a very short immersion in a 0.5 or 1% lithium carbonate solution.

It is known that upon macroscopic examination, the adrenal capsules of the guinea pig show a thin cortical part of a light ochre color and a central part of a dark red color. However, upon microscopic examination, the cells that constitute these two zones appear morphologically identical and nothing I observed to differentiate them. The photographs in plates 22 to 24 concern only the cells of the central zone.

Cytoplasm of the adrenal capsules of the guinea pig

Plates 22, 23, 24 relate to the adrenal capsules of three different guinea pigs. Figures 1 and 2 (1100 enlargement) of Plate 22, as well as Figures 2 and 3 (1100 enlargement) of Plate 23 concern the same adrenal capsule fixed for 4 days by a 12% neutral formalin solution with the addition of 8 p. 1,000 of NaCl and other salts of **Locke's** fluid.

Observation of these figures under a magnifying glass shows the reticulated structure of the cytoplasm, the shape of the dumbbells, and their mode of articulation exclusively by their balls. Despite partial preservation of the cytoplasmic network in many places, the cells are very altered, even vacuolated in places, and the rods of the dumbbells, which have disappeared in different regions, are poorly colored or very slender when preserved; groups of 3, 4, or 5 dumbbell balls articulated together most often still bear a short portion of their rod, which gives these small masses a star-like appearance.

Figs. 3 and 4 of Plate 22 (1100 enlargement), Figure 4 of Plate 23 (1100 enlargement), and Figures 2 and 3 (100 enlargement) of Plate 24 concern the adrenal capsules of another guinea pig fixed for 3 days in a 10% formalin solution with 1 cc of acetic acid and treated for 2 hours, before the paraffin embedding operations, with a mixture of equal volumes of absolute alcohol, ether, and chloroform. Figure 1 of Plate 24 is a photographic enlargement of Figure 2 of this same plate.

The cells of these various figures, whose cytoplasm is altered, nevertheless show the shape of dumbbells, their mode of articulation exclusively by their balls, and the constitution of the cytoplasmic network.

The same applies to figures 5 and 6 of plate 23, which concern the adrenal capsules of a third guinea pig, fixed for 28 hours in a 10% formalin solution with the addition of 0 cc. 5% acetic acid.

Cytoplasm of the rabbit's adrenal capsules

An adrenal capsule cut into thin slices (1 1/2 to 2 mm) is fixed for 3 days in a 12% formalin solution to which the constituent salts of Locke's fluid have been added (formula on page 64). Staining with ferric hematoxylin without differentiation.

Plate 25 shows in Figures 1, 2, 3, and 5 the photograph of cells from the central region of the adrenal capsule at a magnification of approximately 1,600 and in Figure 4, at a magnification of approximately 1,200.

The cytoplasmic network is preserved in these figures, except in a few vacuolated points; but, everywhere, the dumbbells have undergone the alteration of swelling and some of their balls have partially or even completely lost their ability to stain, so that in certain places the rod appears to terminate in a free end.

Note in Figures 4 and 5 the radiating arrangement of the first row of cytoplasmic dumbbells around the nucleus; these two figures show the same field of cells photographed at two different magnifications.

In these figures, the mode of articulation of the dumbbells is clearly distinguished by their balls, but the photographs of the cytoplasm of the cells of the cat's adrenal capsules will provide us with a much clearer and more demonstrative example.

Cytoplasm of the cat's adrenal capsules

Slices 2 millimeters thick from the adrenal capsules, taken immediately after the animal's death, are fixed for 5 days in a 10% formalin solution to which the constituents of **Locke's** fluid have been added, except for dextrose (formula on page 64). By inclusion in paraffin and staining with ferric hematoxylin without differentiation, very beautiful preparations have been obtained in which the cytoplasm is, despite slight alterations, in a state of preservation satisfactory enough to allow the smallest details of its organization to be observed.

Plate 26 shows various photographs of these preparations; Figures 1 and 2 (magnification 1,100) contain groups of cells, in which some points are vacuolated and the dumbbells somewhat altered. We see the strongly stained cytoplasmic network and then, in its meshes and less apparent because they are not in focus, the elements of the deeper regions of this network.

Figs. 3, 4 (magnification 1,100) and 5 (magnification 1,250) show the same cell photographed at three different focusings of the same elements, Figs. 6 and 7, another cell photographed at magnifications of approximately 1,100 and 1,500.

These five photographs show the composition of the cytoplasm of the adrenal capsules in a perfect, almost schematic, way; in the lower half of Figures 3, 4, and 5, the dumbbells are clearly distinguished with their mode of articulation by their balls. At the ape-

xes of certain angles of the meshes of the network, no dumbbell balls are visible because they have become colorless or are destroyed or detached from their organelle.

In the lower half of Figure 7, below the highly colored and superficial meshes of the cytoplasmic network, one can clearly see the existence of other, very clear, although less apparent underlying meshes, which give an exact idea of the morphological constitution of the cytoplasm and the complexity of the network that constitutes it. These images provide indisputable proof of the non-existence of a vacuome constituted by a large number of distinct, isolated cavities, each with its own wall, and of the existence of a single space, occupying the entire cytoplasm and all of whose parts communicate with each other through the large spaces circumscribed by the meshes.

In this space, filled with cytoplasmic fluid, bathes the cytoplasmic network of dumbbells, to the exclusion of other living figurative elements and in particular the debris of the cytoplasm that many authors have described under the name of mitochondria, elements which do not exist, at least as they have been characterized.

In Plate 25, we have observed, in Figures 4 and 5, the radiant arrangement of the cytoplasmic dumbbells at the periphery of the nucleus and the articulation of their central ball with the nuclear membrane. The presence of these balls against the nuclear membrane is made apparent by the scalloping of the periphery of the nucleus.

In the figures in Plates 25 and 26, it will be easily seen under a magnifying glass, at multiple points, that the peripheral dumbbells of the cytoplasm have their external ball articulated with the cell membrane and that their rod always has a direction perpendicular to the section of the latter. By examining this arrangement in many cells, one easily finds some which clearly show that a dumbbell ball belonging to the neighboring cell, or better, comes to lodge in the angular space located between them. There is probably a cement or a film which separates the two cells and is interposed between the balls which belong to their respective cytoplasm. This shows that, in reality, the cell membrane is constituted, at least in the adrenal capsules, both by the balls of the peripheral dumbbells and by this film which separates the balls of the two neighboring cells.

These demonstrations, which are added to those which have already been presented concerning the hepatic cell, provide complete knowledge of the morphological constitution of the cytoplasm and establish:

1. The morphological constitution of the cytoplasmic network of the animal cell by an elementary organelle of a single form, the dumbbell, to the exclusion of all others;

2. The mode of articulation of these dumbbell organelles exclusively by their balls,

either between them to constitute the network, or with the nuclear membrane to fix the nucleus in an immutable position in the middle of the cell, either with the membrane or external cell wall;

3. The existence in the cytoplasm of a single cavity filled with the cytoplasmic liquid, all of whose points communicate freely with each other through the spaces circumscribed by the polygonal meshes of the network;

4. The inexistence of the vacuolar system or apparatus as described up to now and which would be formed of a certain number of isolated, independent cavities of various volumes, filled with liquid;

5. The nature, quality and morphological role of what has been called granular chondriosome and chondrioconte which have been wrongly considered as independent mitochondria and which are, in reality, the balls and rods of the dumbbell organelles, parts which are neither independent nor included in the cytoplasmic network, are definitively fixed and immobile there;

6. The physiological role of the cytoplasmic network in maintaining the nucleus in a fixed position and in preserving the shape of the cell;

7. The fact that all the living matter of the cytoplasm is represented by the substance of the dumbbells of the cytoplasmic network and that the chemical transformations which the cell is the seat take place in these organelles themselves or in contact with them and not through mobile catalysts, the mitochondria, which, in reality, do not exist. The **fig. 1** schematically represents this structure of the cytoplasm and, in addition, that of a nucleus of the enchromocentric type, that is, non-reticulated. The lower left corner of the figure shows two superimposed dumbbell planes.

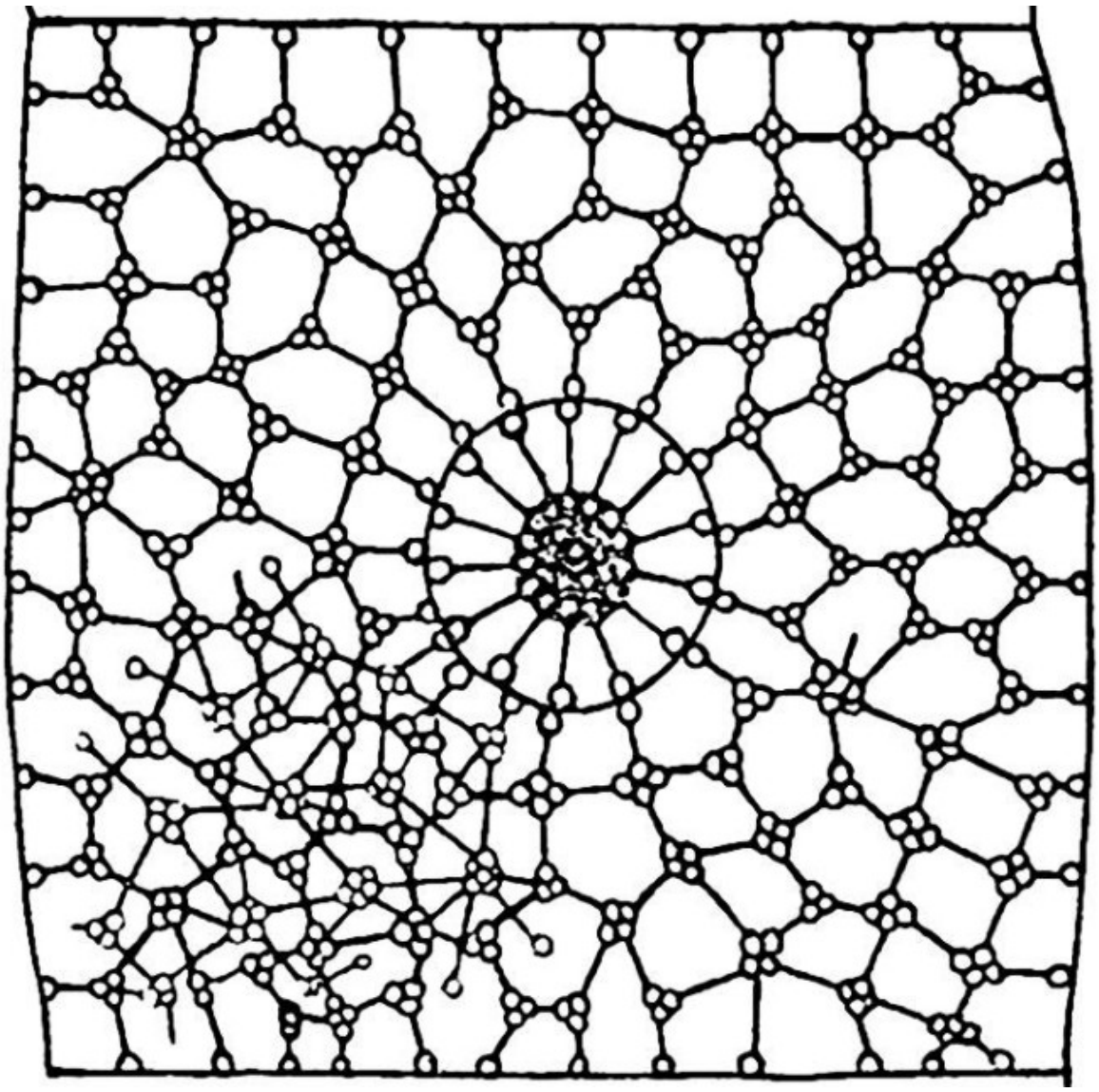

Fig. 1. - Schematic structure of the cell, its cytoplasm and a nucleus of the enchromocentric type.

* * *

CONSTITUTION OF THE CYTOPLASM OF THE CELLS OF THE EPITHELIUM OF THE CONVEYED TUBES OF THE KIDNEY

This study was carried out on the kidney of the guinea pig, the rabbit, the cat, and the dog. But I only obtained appreciable results in the guinea pig and the cat, and then only in exceptional cases because renal tissue is extremely sensitive to the altering action of fixative fluids. Almost consistently, it is observed that the cytoplasmic network no longer exists in the epithelial cell. Only scattered granules are observed in the cytoplasmic space, regardless of the fixative used.

Yvan Bertrand (5), who studied the structure of the cytoplasm of epithelial cells of convoluted tubules in fish, amphibians, reptiles, dogs and rabbits, published in his work a series of beautiful microphotographs in infrared light; in none of the cells photographed is there even the appearance of a cytoplasmic network; it has been destroyed in all cases and only highly swollen granules can be seen in the cells. **Yvan Bertrand** himself admitted that he could not draw any conclusions from this regarding the chondriome, and this, obviously, because of the composition of the fixatives he used.

J. Verne (81), who studied the epithelium of the urinary tubes of lophobranch fish, observed, in the normal state, only numerous granular mitochondria scattered in the basal region of the cell and arranged without any order, or chondriosomes which are arranged in longitudinal sections and take on the appearance of chondrioconts. He then indicates that the action of caffeine causes a vacuolosation of this mitochondrial apparatus.

In reality, the scattered and disordered granular mitochondria in cells in the normal state are certain proof of the total destruction of the cytoplasmic organization, while the vacuolization observed by **J. Verne** in the basal region of the cells after the action of caffeine is the sign of the conservation, in this region, of the cytoplasmic network, which is moreover perfectly recognizable in the drawing he gave of it in figure 7 of the plate of his memoir (pl. XI), a drawing reproduced in figure 2 of plate 28 bis of this volume. We should also point out that the fixation of the pieces studied by **J. Verne** was carried out by the liquids of **Bouin, Flemming, Regaud** and **Carnoy**, which all completely destroy the cytoplasmic network of the epithelial cells of the urinary tubes.

As for the pieces studied by **Yvan Bertrand** (5), they were treated with **Bouin** or **Saner** fixatives (absolute alcohol 6, chloroform 3, acetic acid 1), both of which also destroy the cytoplasmic network of kidney cells.

Formalin, used alone or with a small amount of acetic acid (less than 1%), also alters the cytoplasmic network, but not sufficiently to prevent its existence. When the constituents of **Locke's** fluid are added to its solution, it appears to be less altering. But in such matters, it is difficult to give precise indications, because the results obtained are so irregular that it is very difficult to explain them. Having obtained the results shown in plates 28 and 28a on the cat kidney, the same process applied under exactly the same conditions gave negative results in six other consecutive tests. It was impossible for me to obtain preservation of the cytoplasmic network in the rabbit.

Cytoplasm of the epithelial cells of the convoluted tubules of the guinea pig kidney.

Plate 27 contains, in figures 1, 3, 4, and 5 (magnification 1,100), photographs of sections of a guinea pig kidney fixed for 54 hours with a 10% formalin solution supplemented with 0 cc. 7% acetic acid and, in figure 2 (magnification 1,100), a section of a kidney from another guinea pig fixed for 4 days with a formalin solution alone.

In these various figures, we see an altered cytoplasmic network, but one that is nevertheless very clear and whose existence cannot be doubted. Only the clarity of the dumbbells leaves something to be desired. In addition, we see in Figure 2 a nucleus whose organization has been well preserved and in which we can see that the large chromatic masses are the points of articulation of a group of dumbbell balls whose rods give these masses a star-like appearance, the other ball of each dumbbell often being visible, either in a neighboring chromatic mass or attached to the nuclear wall. This nucleus will be discussed again later.

We will only retain from the examination of the photographs in Plate 27 the demonstration they provide of the existence of the cytoplasmic network with its characteristic shape, in the cytoplasm of the cells of the epithelium of the convoluted tubules of the guinea pig kidney. Examination of these same cells in the cat kidney will provide us with much more precise information.

Cytoplasm of the epithelial cells of the convoluted tubules of the cat kidney

Thin slices of the kidney (1 to 2 mm) taken immediately after death by section of the bulb were fixed for 68 hours in a 12% formalin solution (figs. 2, 5, 6, pl. 28a, and figs. 1 to 4, pl. 28) to which the constituents of **Locke's** fluid were added. The very thin sections (212) were stained with ferric hematoxylin without differentiation and passed quickly into a 1% lithium carbonate solution.

The photographs in plates 28 and 28a show a strongly altered cytoplasmic network, destroyed in many places, but well enough preserved in others for its existence and characteristics to be established with certainty and precision.

By examining the photographs with a magnifying glass, it will be easy to see that the polygonal meshes of the network are made up of dumbbells that are articulated together exclusively by their balls and that these are always located at the vertices of the angles of the polygons, the sides of these being made up of the rods of these same dumbbells. These devices will be found very clear and very demonstrative in figures 2, 3

and 4 (approximately 750 magnification). In these figures, the cells have undergone a swelling that has flattened the central canal (except in figure 1, 750 magnification). The convoluted tubes have, in these figures, an elliptical shape because the section is made obliquely.

In Plate 28a, Figure 3 contains a group of convoluted tubules, at a magnification of approximately 325, of which Figure 1 shows the three lower ones photographed at a magnification of 750. In the central convoluted tubule of this figure, we will notice in the upper region a nucleus around which the cytoplasmic dumbbells have a radiating arrangement. We will see this same arrangement in Figures 5 and 6 of this same plate, representing the same cell photographed at a magnification of 1,500 with two different focuses. We can also see there, particularly in Figure 5, a portion of the cytoplasmic network. Figure 4 shows the section of a convoluted tubule in which we can still see, despite advanced destruction, portions of the cytoplasmic network that are still clear.

These various photographs therefore clearly establish that the network of the two cells in Figure 2, Plate 28a (**J. Verne**), is indeed the normal cytoplasmic network and does not result from a process of vacuolation.

Plates 27, 28, and 28a therefore show the cytoplasmic network in the guinea pig and the cat, and thus establish that the cells of the epithelium of the urinary tubules of the kidney have a cytoplasmic organization identical to that of the hepatic cells and that of the adrenal capsules. Thus, it is clear that when the cytoplasmic network does not exist in drawings or photographs of epithelial cells of the kidney, it is because it has been destroyed. This is the case, for example, with the microphotographs in **Yvan Bertrand's** publication (1 bis).

By comparing these microphotographs with those contained in the plates of this work, it will be possible to judge that photography with white light gives results at least equal to those provided by infrared photography.

It can be stated that the results obtained depend not on the photographic process, but exclusively on the quality of the preparation examined, its sharpness, and the degree of preservation of the treated tissue.

CONSTITUTION OF THE CYTOPLASM OF SPINAL GANGLIA CELLS

The best results I have been able to achieve in the study of these cells have been obtained with the spinal ganglia of a cariacou (*Odocoileus virginianus)* taken only three or four hours after the animal's death (a death whose cause could not be determined by autopsy), that is, within a period when no significant alterations occurred in the spinal nerves and their ganglia. Fixation was done in a 10% formalin solution for 3 days, inclu-

sion in paraffin and staining with ferric hematoxylin for figures 1 to 4 plates 16 and 4, 5, plate 16 bis.

Figs. 4 of Pl. 16 (magnification 325) and 5 of Pl. 16a (magnification 325), show groups of cells whose cytoplasm has undergone a certain retraction due mainly to the action of alcohol and toluene. Figures 1 and 2, Plate 16 show, at a magnification of 1,650, the upper part of the cytoplasm of the large upper left cell of Figure 4, photographed at two different focuses; Figure 3 is a photo- graph of the lower part of this same cell.

These two photographs show that the cell's cytoplasm is made up exclusively of dumbbell organelles articulated together by their balls to form a very fine network with polygonal meshes. This network appears more clearly in Figure 1 than in Figure 2, and in both photographs, its sharpness is somewhat diminished by the swelling of the balls of the dumbbells, which, moreover, are very slightly colored, sometimes even colorless. In Figure 4 of Plate 16a, we see, at a magnification of 1,650, the cytoplasmic network of another cell, but less clear than in the previous figures.

In the latter, we notice some white spaces which are certainly due to the shrinkage caused by alcohol and toluene.

We see, therefore, by examining the photographs in plate 16, that the cytoplasm of the cells of the ganglia of the spinal nerves does not contain canals corresponding to those that **Holmgren** (13) reported in these cells and which together would constitute the apparatus described by this author under the name of Trophospongium.

Figs. 1 and 2 of plate 16a are photographs of figs. 2 and 4 of the text of E. Holmgren's memoir (13). They show two cells of ganglia of the spinal nerves whose cytoplasm appears to be exclusively composed of granules which are the balls of the dumbbells shown in plate 16. But these cells appear to be deprived of any network, firstly because it is an obviously incomplete drawing and secondly because the balls of the dumbbells are greatly swollen and their rods probably destroyed.

In Figure 3 of Plate 16a, some figures from Plate 4 of **Holmgren's** memoir (13) are reproduced. In the lower left corner of the figure, we see a cell with a network-like appearance which, quite obviously, only responds to a destruction of the dumbbells by the fixative, resulting in a large vacuolization made certain by the existence of the two larger vacuoles located in the upper right part of the cell; the two cells in the upper part of this Figure 3 present the same vacuolization also detected by the appearance of canals in the two middle cells. Indeed, in the figures of our Plate 16, where the dumbbells, although swollen, are perfectly preserved, we do not notice any image of this kind, apart from a few voids due to retraction.

Moreover, the existence of a single cytoplasmic space or cavity whose points all communicate freely with each other is further proof of the non-existence of these channels which, by this fact, are perfectly useless since there is free communication everywhere between the nucleus and the cell membrane.

The figures in plates 16 and 16a therefore clearly demonstrate the non-existence of the system of canals described by **Holmgren** (Trophopongium).

Figures 1, 2, 3 of plate 16 certainly demonstrate the existence of a very fine-mesh network, but which has a completely different appearance from the cytoplasmic network of liver cells and adrenal capsules, for example.

This comes from the fact that this fine network results from the deployment in the cytoplasmic space of the neurofibrils of a large afferent nerve fiber, their anastomosis and the multiplication of their dumbbells, this resulting in the reconstitution of the neurofibrils of the cylinder-axis of the efferent nerve fiber.

Now, we will see, in studying the constitution of the neuron, that the perinuclear space of nerve cells, pyramidal cells, of the cerebral cortex for example, occupied by neurofibrils is not a cytoplasm, this being the pericellular network of Golgi. It is very unlikely that the perinuclear network of the cells of the spinal ganglia, constituted exclusively by the dumbbells of neurofibrils coming from the outside, is truly a cytoplasm; this explains why this network has a different appearance from the cytoplasmic network of the cells of other organs.

If it is not a cytoplasm, which seems probable, it is equally probable that there exists, at the periphery of the ganglion cell, a network identical to the pericellular network whose existence Golgi demonstrated at the periphery of the short-axis cylinder cells of the third layer of the gray matter of the cerebral cortex (see p. 95 and pl. 37 bis). This fact is all the more probable since the efferent nerve fiber is covered with a myelin sheath as soon as it leaves, and it is demonstrated later that this is of a cytoplasmic nature (see p. 95).

The fact remains that this dumbbell network exists and is made up of open meshes allowing free passage of the internal liquid throughout the perinuclear space, a fact which, by establishing the uselessness of **Holmgren's** canal system, completes the demonstration of its non-existence.

* * *

According to current knowledge, myelin nerve fibers are essentially composed of:

1. A thick sheath whose framework is made up of a network of fine tracts, the neurokeratin network, discovered by **Ewald** and **Kühne** in 1877; the spaces between these tracts are filled with a soft lipoid substance, myelin, including the spaces which led to this part of the nerve fiber being called the myelin sheath;

2. A bundle of neurofibrils located in the center of the fiber, the axial cylinder, whose elements have a direction parallel to the axis of the fiber and which is completely surrounded by the myelin sheath forming a sleeve around it;

3. A very thin membrane known as anhist, the **Schwann** sheath, attached to the outer face of the myelin sheath.

We will study each of the first two parts.

Myelin Sheath

In addition to its special constitution by the network of tracts that I have just mentioned, this sheath presents the following anatomical peculiarities that we will study later:

1. The interannular constrictions discovered by **Ranvier**;
2. The incisures of **Schmidt** (1874) and **Lanterman** (1876);
3. The conical membranes that enter into the constitution of these incisures.

I do not note here, among these anatomical peculiarities, the lamellar or laminated structure described by **Ranvier** and others, because it has no real existence; it is, when observed, an artifact due to the method of preparation of the nerve. We will first deal with the neurokeratin network that constitutes the supporting framework of the myelin sheath.

* * *

Neurokeratin Network

This network was discovered by **Ewald** and **Kühne** (19) in 1877. The authors who studied it after them added nothing new to the question. They described in this network an external horny sheath and an internal horny sheath, the latter applied against the axial cylinder, then a network of radiating and branched trabeculae connecting the external and internal horny sheaths together.

During a study aimed at finding a way to chemically differentiate tissues and which consisted of subjecting them to the digestive action of pancreatic juice, **Ewald** and **Kühne** saw that nerve fibers and nervous tissue in general offered great resistance to this digestion; by studying the residue of this digestion, they recognized that it consisted of a network formed by a substance presenting certain characteristics of the horny

substance and which neither pancreatin, nor pepsin, nor potash, nor sulfuric acid attacked, and to which they gave the name neurokeratin.

Then these authors indicated another process for highlighting this substance in the myelinated nerve fibers of peripheral nerves, a process consisting of treating them successively for 24 hours with absolute alcohol, then 2 hours with boiling alcohol and finally 24 hours with ether, a treatment which has the effect of dissolving the myelin and making a particular network, the neurokeratin network, visible in the nerve fiber. This network is, according to them, made up of a thin external horny sheath, a thin internal horny sheath applied against the cylinder-axis and a network of branched trabeculae and radiating connecting these two horny sheaths.

Note here that it is the entire myelin sheath, including the internal and external sheaths, that **Ewald** and **Kühne** described under the name of neurokeratin network.

The same year, **Lanterman** (37) also made known the existence of the network that bears his name, in just a few lines, the literal translation of which is as follows:

Finally, I must draw attention to the elegant reticulated pattern which often appears with great regularity in osmic acid preparations; I have not been able to specify until now what it signifies. Regarding the research procedure, I indicate that the whole fresh nerve is immersed in a 1 part osmic acid solution for 15 to 30 minutes; one or two hours are necessary for more dilute solutions.

Thus, what **Lanterman's** drawing, reproduced in Figure 1, Plate 29, represents is only the external part of the neurokeratin network, that is, its thin external sheath, because it is this alone that is visible on a fresh nerve treated with osmic acid.

Various authors have considered the neurokeratin network to be an artificial product resulting from the action of the reagents, as an artifact, in a word. This was the opinion of **Ranvier,** who explained the artificial production of this network by the formation, in the myelin, by the effect of the treatment, of large balls between which the remaining substance, once fixed, would necessarily take on the appearance of a network.

Hesse (32) also contested the real existence of this network and expressed an opinion similar to that of **Ranvier**, claiming that the horny substance does not normally exist in the state of a network, but mixed everywhere with the myelin in a homogeneous manner.

On the contrary, **Tizzoni** (79) and **Golgi** (25) confirmed the existence of this network of horny substance.

Waldstein and **Weber** (82), who reviewed the research of **Ewald** and **Kühne**, considered the neurokeratin network to be an artificial, non-preformed formation in the nerve, but provided no convincing evidence.

They provided very clear images (drawings) (figs. 2 and 3, pl. 29) which correspond to the outer sheath of the neurokeratin network and to the **Lanterman** network.

Here are the arguments invoked by **Waldstein** and **Weber** to justify their conclusions:

The knotty network that is manifested inside the nerves, always more or less the same for the same animal when obtained by classical methods, is far from being the same if it is produced simply by the action of alcohol. It then differs according to whether the alcohol is more or less diluted and according to the time that has elapsed from the death of the animal until the moment at which the nerve was taken to place it in the reagent.

Finally, if instead of resorting to the combined treatment with alcohol and ether, the fresh nerve is treated directly with ether, no network is obtained at all, or at most, some traces of it are observed.

These considerable varieties in the form of the network according to the conditions in which it is manifested already suggest that it is not preformed in the nerve.

*If we add that after the action of water on the fresh nerve, no framework can be manifested by classical treatment and that only scattered rods are obtained in its place, we must conclude that the knotty framework or the so-called horny network (**Horngerüst**) is formed inside the nerve by the dissociation of the myelin under the influence of the reagents into two substances: a fatty substance dissolved by ether, and another substance, the nature of which we do not dwell on, which takes various forms inside the nerve tube.*

It would even appear from our digestion experiments that the myelin contains, besides fat, two different substances, one which is digested by pancreatin, the other which remains in the form of refractile rods.

*What confirms the opinion we have just expressed on the nature of the horny network are the experiments carried out on degenerated nerves. **Kühne** (4, p. 260) says incidentally in this regard that the knotty network still appears at the points where the myelin begins to*

*disappear. This is true, and the reservation is prudent. But, as **Pertik** has pointed out, when the degeneration is well marked, the horny network no longer occurs except at the points where there was still myelin... Finally, a final proof that the knotty network is formed at the expense of myelin is that it can be produced, as **Hesse** (p. 362) and **Pertik** (p. 231) have already pointed out, on the myelin that has emerged from the nerve tubes and spread out on the glass slide, by treating it using the classical method. We repeated these experiments and were able to obtain in the irregular balls of myelin spread on a glass slide, a real network, analogous to that shown by **Pertik**.*

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The arguments invoked by **Waldstein** and **Weber** in no way diminish the value of the conclusions of **Ewald** and **Kühne,** and in no way prove that the neurokeratin network does not normally exist in the myelinated nerve fiber.

The differences in the appearance of the network that result from a modification of the treatment, either by absolute alcohol alone, or by diluted alcohol, or by ether alone, were clearly observed by **Ewald** and **Kühne**, and they arise from the fact that, in these different cases, the solubility of the myelin in the solvent is incomplete and variable. If **Ewald** and **Kühne** adopted a treatment involving the successive action of the two solvents, it is understandable that it is precisely because they clearly observed these differences in solubility.

The absence of a network in the fiber after treatment with water is not surprising since we know, for example, that pure water dissociates, disorganizes, and even destroys quite quickly the cytoplasm of cells, which is made up of a network similar to the neurokeratin network. We will see later that the myelin sheath is, in short, the cytoplasm of the myelinated nerve fiber and that the neurokeratin network is the homologue of the cytoplasmic network of cellular elements.

As for the presence of a network in the myelin droplets that escaped from the cut nerve fiber, for the fact to have any significance, it would be necessary to demonstrate that this network was not carried out of the nerve fiber with the drop of myelin. It would also be necessary to demonstrate, with good photographs, that the network of these droplets is indeed identical to the neurokeratin network and formed, like the latter, by dumbbell-shaped organelles, a fact that has not been demonstrated.

Nageotte expressed the following opinion on the neurokeratin network on page 250 of his book (52): *The Organization of Matter*:

...The neurokeratin network has recently been considered a normal arrangement of the sheath and as containing myelin itself within its meshes: two errors that I have refuted.

Neurokeratin therefore results from a dissociation by which one of the substances that enter into the composition of the myelin sheath separates from the others.

Nageotte therefore does not admit the material, real existence of the neurokeratin network and he considers it to be a newly formed network resulting from the action of substances that act on the nerve during its preparation. This opinion of **Nageotte** will be discussed later after we have studied the characteristics of the neurokeratin network; this study will make it easier to see why **Nageotte's** opinion on this matter is unfounded. We can judge from all the research that follows and from all the demonstrations given in plates 29 to 39 that:

1. The neurokeratin network is not an artifact; it has a real existence; it is the framework or even the skeleton of the myelin sheath and we can even say that it is this sheath itself, the latter comprising at the same time an external membrane, an internal membrane surrounding the cylinder-axis and the middle zone connecting these two membranes;

2. Lanterman's network is the neurokeratin network itself, colored black by osmic acid, but it is only a part of it; it corresponds only to the external sheath of the network. Indeed, what **Lanterman** drew (fig. 1, plate 29) is only this external sheath, whereas the middle zone of the sheath has a completely different appearance;

3. The neurokeratin network reduces osmic acid when it is made to appear, as **Lanterman** did, by immersing the entire fresh nerve in a 1% osmic acid solution. In this case, the neurokeratin network becomes colored a more or less dark gray, representing the image very faithfully reproduced by **Lanterman's** drawing, which can be found photographed in figure 1 of plate 29.

As seen in **Lanterman's** drawing, the spaces circumscribed by the meshes of the network remain white, at least at the beginning of the treatment, which proves that the osmosis-reducing substance is contained in the network itself and, more precisely, in the rods that form this network. But, over time, the white spaces circumscribed by the meshes also end up turning gray, which further proves that the substance contained in these meshes, myelin, also contains an osmosis-reducing substance, perhaps the same one, but that it contains a smaller proportion than the neurokeratin network.

It was wrongly believed that certain characteristics could differentiate these two networks. For example, the **Lanterman** network appears after treatment of the fresh nerve with osmic acid, while the neurokeratin network is no longer colored by osmic acid after treatment of the nerve with solvents of lipoid materials, turpentine, alcohol, ether, toluene, etc. Does this establish a difference between the two networks?

Not at all. This difference only proves that the elements that constitute the neurokeratin network contain an osmio-reductive substance and that, when this substance that caused the coloring is removed with a solvent, osmic acid can no longer stain the network.

Indeed, the neurokeratin network, after treatment of the nerve with myelin solvents, stains very easily with ferric hematoxylin and with aniline colors, acid fuchsin, Ziehl, etc. It stains just as well with these dyes in the fresh and intact nerve, that is, one not treated with lipoid solvents.

But, in this case, the network is barely visible because it is masked by the opacity of the myelin, which also fixes the dye.

On page 248 of his book *The Organization of Matter*, **Nageotte** (52) wrote, in the caption to Figure 34, about the ***C*** fiber fixed in formalin and treated with alcohol, then stained with acid fuchsin: "*The osmiophilic substance swelled and dissolved, the neurokeratin network formed.*" This statement does not correspond to the reality of the facts; the neurokeratin network did not form, because it normally existed in the fresh, intact fiber. The alcohol treatment, by dissolving the myelin, only made it apparent, because the opaque myelin no longer masks it.

The **Lanterman** network therefore does not have the characteristic of being colored or developed by the action of osmic acid. When it is degreased, it remains colorable by aniline dyes or hematoxylin and, thus colored, it is still in the same way the **Lanterman** network. This also applies to the entire neurokeratin network.

On page 248, **Nageotte** writes: "*The neurokeratin network is nothing other than the degreased **Lanterman** network; these two artifacts are formed by the same mechanism*."

This is also inaccurate. This opinion is not consistent with the facts; the neurokeratin network and the **Lanterman** network, whether degreased or not, are one and the same thing; the non-degreased **Lanterman** network is also the neurokeratin network; the two expressions designate the same object, with the distinction that the neurokeratin network designates the entire supporting framework, that is, the entire myelin sheath, while the **Lanterman** network designates only the external membrane; the latter, as we will see later, has its constituent organelles located on the same cylindrical surface, while the middle zone has its elements oriented in an infinite number of different planes. Moreover, **Nageotte's** assertion is inaccurate because these two networks, which correspond to only one, are not an artifact, but have a real existence.

Here it becomes necessary to define what is meant by an artifact; it must be considered in its strict sense, as existing materially and as being an accidental, fortuitous arrangement of living matter, having no relation to normal anatomical formations and whose

cause is attributable exclusively either to post-mortem physicochemical phenomena or to the treatment undergone by the tissue in its preparation for microscopic examination (action of fixatives, alcohol, toluene, water, heat, etc.).

* * *

After this historical and critical overview, let us now move on to the description of our own observations relating to the myelin sheath: This study was carried out on myelinated fibers from the rabbit sciatic nerve fixed for 2 to 4 days with a 5 to 10% formalin solution or with **Locke's** fluid containing a proportion of 3 to 12% formalin depending on the case **(1)**. The fixed nerve is dehydrated for about 2 hours with alcohol, then two more hours with toluene, and finally embedded in paraffin. The sections are stained with ferric hematoxylin, sometimes adding a second staining with Ziehl fuchsin diluted with 1/3 or 2/3 90% alcohol. This treatment indicates that the nerve has been subjected to two solvents of the nerve's lipoids, that is, myelin, and explains why the sections show clear sharp networks.

However, the lipoid substances are not always completely dissolved, and often a small quantity remains, which clogs the dumbbells and especially the articulation points of their balls.

In a successful preparation, such as the one for which a number of photographed fields are represented in plates 29, 30, and 34, the nerve fibers all show the neurokeratin network, either by the external sheath (**Lanterman** network) (pl. 29, figs. 6, 7, and 8; pl. 30, figs. 1 to 5), or by its internal sheath (pl. 34, figs. 4 and 5), or by its middle part (network of radiating tracts) (pl. 29, figs. 6, 7, and 8; pl. 30, fig. 5, fiber h). Let us examine each of these parts.

OUTER SHEATH OF THE NEUROKERATIN NETWORK

In Plate 29 we find:

1. In Figure 1, the photograph of **Lanterman's** drawing (14) showing the network that bears his name;

2. In Figures 2 and 3, drawings by **Waldstein** and **Weber** (36) representing the neurokeratin network obtained by the **Ewald** and **Kühne** process;

3. In Figure 4, the photograph of Figure 35 from **Nageotte's** book (22) showing in the right-hand fibers the external sheath of the neurokeratin network or **Lanterman**

(1) This percentage is counted in absolute value of formic aldehyde like 40% formalin, for example; the 10% solution therefore corresponds to 40% formalin diluted ¼.

network seen from the front and in the second fiber from the left, cut through its middle, the radiating tracts of the middle layer of the myelin sheath;

4. In Fig. 5, a photograph of Figure 34 from **Nageotte's** book representing a drawing of **Lanterman's** network colored by acid fuchsin after the action of alcohol on the sciatica of a rabbit fixed by formalin;

5. In Figure 6, the photograph of a section of rabbit sciatica fixed by formalin and stained by ferric hematoxylin showing the outer sheath and the middle layer of the myelin sheath with its radiating tracts;

6. Figure 7 shows the same image photographed with twice the magnification (1.100). This photograph shows on the left, in two obliquely cut nerve fibers, a very small portion of the outer sheath, at the level where it is cut, then a portion of the cylinder-axis with the radiating tracts that connect the inner sheath, which is not seen here and which is attached to the cylinder-axis, to the outer sheath (**Lanterman's** network). Here, it is clearly visible that these tracts are radiating, simple, unbranched, and constituted, as is frequently the case, by a single dumbbell, one ball of which is attached to the cylinder-axis and the other to the outer sheath. The fibers in the right half of the figure show the outer sheath or **Lanterman's** network.

It is clearly apparent in these two fibers that the purpose of the radiating tracts is to maintain the cylinder-axis in a fixed position at the center of the fiber, that is to say, in its axis, and that, being firmly articulated by their balls, on the one hand to the internal sheath glued against the cylinder-axis and on the other hand to the external sheath (**Lanterman** network), the whole of these three parts constitutes a single network which is the support framework of the myelin sheath, a framework which has a triple effect:

A) To ensure the strength of this sheath while retaining all the necessary elasticity and flexibility and thus preventing its deformation during voluntary movements; this result is obtained by the admirable process of articulating the dumbbells with each other by their balls, a process which is, in short, the most perfect mode of articulation of levers from a mechanical point of view: the ball joint. The adhesive, sticky surface of the balls performs the role of maintaining or fixing the female part of the ball joint, the ball of the dumbbell constitutes the male part and its median rod is the lever whose ball joint allows movement in all directions.

This mode of articulation is general in the entire animal or plant organism, whose cellular elements are constituted entirely by the dumbbell organelle which thus appears as the agent of organization of life.

B) To leave between its meshes the gaps necessary for the accommodation of the myelin.

C) To ensure that the cylinder-axis is kept in a fixed position along the axis of the fiber, especially during voluntary movements;

7. Figure 8 shows two nerve fibers cut obliquely in which we see: in the left fiber the external sheath (Lanterman network) at the top and, more below, the cylinder-axis with its radiating tracts constituted by a single dumbbell; in the right fiber two portions of external sheath in the upper part with, in the middle part, a deformed interannular constriction.

Plate 29 therefore already shows us the appearance and constitution of the external sheath (**Lanterman** network) of the myelin sheath as well as one of the aspects of the middle zone of this sheath and the nature of the elements which constitute it.

Plate 30 gives the same demonstrations in a very satisfactory way. It shows that the outer sheath (**Lanterman** network) is composed of polygonal meshes of variable size for the same fiber and from one fiber to another. In the second fiber from the left in Fig. 4, we see a network with very large meshes, while the fiber in Fig. 1 has a net- work with smaller meshes. This difference is probably due, at least for certain regions, to the degree of alteration by the fixative which attacks and progressively reduces the rods of the dumbbells, which causes a progressive vacuolization whose effects can be seen in Fig. 2, for example, by the two vacuoles which exist at the bottom of the figure, one of which is in fiber ***C***. Let us first note that, without a shadow of a doubt and quite obviously, this network is the **Lanterman** network.

Let us then note that, whatever part of the neurokeratin network is in view, it is constituted in its entirety by dumbbells which form the sides of the polygonal meshes, the vertices of the angles of these polygons being the points of articulation of the balls of the dumbbells. These latter and their mode of articulation will be seen by carefully examining, under a magnifying glass, all the nerve fibers in Figures 1 to 5 of Plate 30.

They will also be seen in Figures 1 and 2 of Plate 35 which represent, photographed at a magnification of 1,100, portions of the external sheath (**Lanterman** network) of the rabbit sciatic nerve and which clearly show the mode of articulation of the dumbbells by their balls. This mode of articulation will also be seen in fibers ***b*** and ***c*** of Figure 2, Plate 30, which both show the intermediate network, that is, the middle layer of the neurokeratin network cut between the cylinder-axis and the external sheath.

In fiber ***f,*** at the connection points of the middle third with the upper and lower thirds, we see two regions in which the meshes of the network are much smaller, very fine; some appear to be included in the large ones, which they subdivide into a secondary network of smaller meshes also formed by tiny dumbbells. We will also see these regions of fine networks in fibers a, figure 1, ***d, e***, figure 3 and figure 5.

By carefully exami- ning these different regions with a magnifying glass, we will clearly see the dumbbell shape of the tiny organelles that constitute these fine networks; these regions are the connection points with the external network of conical membranes that constitute the **Schmidt-Lanterman** notches; the fineness of these networks of tiny dumbbells is one of the characteristics of these conical membranes that we will study later.

It will also be observed that these dumbbells are of a paler shade than the meshes of the outer sheath, and this is because, being located at a different level, either lower or higher, they cannot be brought into exact focus and consequently appear with a paler shade in the photograph.

In Figure 6, we will see a very large nerve fiber which shows at ***k*** the network of the middle layer of the myelin sheath, a very fine network and very branched in all directions whose tracts are connected to the cylinder-axis ***e***, the left side of the fiber, higher, showing only the radiating tracts formed by one or two dumbbells only.

MIDDLE OR INTERMEDIATE LAYER OF THE NEUROKERATIN NETWORK

This is a layer that **Ewald** and **Kühne** (19) described in 1887 as formed by branched radiating tracts, connecting the network of the inner horny sheath, which lies against the cylinder axis, to the network of the outer horny sheath, which is also **Lanterman's** network.

In his book *The Organization of Matter* (Paris, 1923), **Nageotte** (52) describes these tracts again as follows (page 227):

If, after treating a nerve with acetic bichromate and making thin transverse sections, it is stained with iron hematoxylin, a remarkable image is obtained.

The cylindraxis is more or less retracted depending on the dose of acetic acid; it has taken on the star-like shape, which has been known for a long time and whose significance has not yet been understood. In these preparations, the reason for this deformation of the cylindraxis becomes very clear: the branches of the stars are continued by fine tracts divided dichotomously, which cross the entire thickness of the myelin sheath to fix it at its external limit; these tracts are very numerous; they constitute an elegant radiated formation...

Further on, page 229, he completes his description thus:

*If the preparation is stained after fixation with acetic bichromate, by the **Altmann** method, or even by that of **Benda**, we see appearing, in place of these branched tracts, an enormous*

quantity of granular filaments, oblique in various directions, which extend from the cylindrical axis to the periphery of the fiber. These filaments, which resemble bacilli, are very clearly individualized; they terminate freely at each of their extremities. Comparison with the previous figure shows that they are located in the radiating and branched tracts described above; but they remain simple and follow a more or less curvilinear path, engaging, at each bifurcation, in only one of the two branches of the protoplasmic tract to which they belong (fig. 21, a, c, e; figs. 22 and 23). These filaments are mitochondria, there is no doubt about it, and the tracts in which they are placed are continuous with the neurite... etc.

Let us first note that **Nageotte's** description coincides exactly with the observations of **Ewald** and **Kühne** and that it does indeed concern the network of branched and radiating trabeculae connecting the external horny sheath to the internal horny sheath described by these two observers. This conclusion is also consistent with other statements by Nagcotte. On page 232 of his book (22), he writes, in essence (11th line): "*The **Lanterman** network of well-fixed fibers is, in essence, only an image of mitochondria*." Further on, on page 248, we read: "*The neurokeratin network is nothing other than the defatted **Lanterman** network*."

Thus, mitochondria, **Lanterman's** network, and neurokeratin network are, according to **Nageotte**, one and the same thing, and his assertions prove that he has indeed assimilated the network of branched tracts he observed to **Ewald** and **Kühne's** neurokeratin network.

But his last observation is not exact. The neurokeratin network is not the defatted **Lanterman's** network; it is also the non-defatted **Lanterman's** network; there is only one network existing in the living nerve; **Lanterman**, by treating the fresh nerve with a weak solution of osmic acid, saw only the dark gray superficial part of this network (because on a fresh nerve, one can only see what exists at the periphery) which bears his name; **Ewald** and **Kühne** had observed their neurokeratin network the same year by treating the nerve with lipoid solvents. The fact that the network no longer had, in this second case, the same chemical composition as in the first, does not imply in any way that it is not the same in both cases. But in this second case, the defatting of the myelin fiber had the effect of allowing **Ewald** and **Kühne** to see much more than what **Lanterman** had seen: they saw the entire interior of the myelin sheath, that is to say, in addition to the external horny sheath or superficial network, the middle network of branched radiating tracts and the internal horny sheath, a network applied against the cylinder-axis. These last two networks cannot be seen on the fresh nerve because they are hidden there by the opacity of the myelin.

The difference between the **Lanterman** network and the neurokeratin network is therefore not, as **Nageotte** claims, that the latter is degreased. The essential difference consists in the fact that **Lanterman** saw only the superficial part of the network, while **Ewald** and **Kühne** saw, in addition, the middle zone of branched tracts and the internal sheath or network applied against the cylinder-axis, a network which, to my knowledge, was not seen by anyone other than them; it is the latter network shown in Figures 4 and 5, Plate 34.

After these explanations, one will be surprised that, after having affirmed that the neuro keratin network is nothing other than the degreased **Lanterman** network, **Nageotte** added this sentence (p. 248): "*These two artifacts are formed by the same mechanism,*" then, on page 250:

Under the influence of crushing, the axis cylinder loses its constituent water, which spreads into the myelin sheath and dissociates it into an irregular network. This network, which is in reality only the neurokeratin network, has recently been considered as a normal arrangement of the sheath and as containing the myelin itself within its meshes: two errors that I have refuted. Neurokeratin therefore results from a dissociation by which one of the substances that enter into the composition of the myelin sheath separates from the others. This substance acquires, under the influence of fixatives, a considerable resistance, as is known, to all agents of destruction... The resistance of neurokeratin reaches an extreme degree when fixation is obtained using formalin... etc.

Let us first rectify this last statement, which is inaccurate; fixatives attack and destroy the neurokeratin network; formalin itself, far from giving it resistance, attacks it.

Let us then note how incomprehensible are **Nageotte's** assertions that the **Lanterman** network and the neurokeratin network are two artifacts, that is, that it is an error to claim that they are a normal arrangement of the nerve fiber.

How can **Nageotte** explain these negations when, after having shown the existence of the network of branched tracts and mitochondria **(1)**, which he finds impossible to qualify as an artifact, he specifies that "*The **Lanterman** network of fibers well fixed by osmic acid is only an image of the mitochondria*."

Nageotte claims that the neurokeratin network does not pre-exist and is formed only when the nerve fiber is treated with a solvent that dissolves a certain osmosis-reducing substance mixed with the myelin; it is this separation of this substance from the myelin that creates the network.

(1) *Which, obviously, is the intermediate network of Ewald and Kühne, i.e., part of the neurokeratin network.*

There are two factual errors in this explanation:

1. The first is that the solvent does not dissociate the myelin to remove an osmio-reducing substance: it removes all the myelin; the meshes of the neurokeratin network are completely emptied of it, a fact that is easy to verify in plate 30, for example;

2. The network itself, which obviously contains an osmiophilic substance, since osmic acid blackens it, is deprived of it by the solvent, but this in no way gives it the characteristics of an artifact and it remains just as easily stainable by other processes. The treatment of the nerve with myelin solvents is a judicious artifice that is not intended to create special devices that do not exist in the normal nerve. Its only effect is to remove from the nerve an opaque substance that masks its internal arrangements.

We will conclude this discussion by concluding that the neurokeratin network is a normal and essential, fundamental device of the living nerve; that it is not an artifact and that it occupies the entire space of the myelin sheath, forming an outer sheath, an intermediate network, and an inner sheath, while **Lanterman's** network, as he described it in his first memoir, is only the superficial and external network.

Finally, let us add that the neurokeratin network possesses a characteristic that had never been noticed until now and which eliminates any discussion about its real existence and quality: that is, it is constituted by dumbbell organelles articulated together by their balls, a fact which indicates that it is part of the living matter of the nerve fiber.

* * *

After this discussion, let us continue the study of the intermediate zone of the neurokeratin network. Let us first note that the branched tracts that **Nageotte** described and into which the mitochondrial rods would engage do not really exist; they are due either to the persistence of a certain quantity of undissolved myelin around the rods, or to a coagulum that accumulates around the dumbbell organelles of the network; the clarity of this deposit, as well as that of the dumbbells it covers, is notably diminished by the altering action of the fixative, an action very accentuated in the case of the acetic dichromate used by **Nageotte**. In reality, only the mitochondrial rods exist which, according to **Nageotte**, would engage in the branched and hollow tracts. **Nageotte** describes these rods thus:

These filaments, which resemble bacilli, are very clearly individualized; they terminate freely at each of their ends... These filaments are mitochondria, there is no doubt about it.

This description proves that **Nageotte** did not see that these filaments are dumbbells. It will be shown later that their ends are never free and that they always terminate in

a ball articulated with another dumbbell, particularly at the level of the internal and external networks.

This network has very different appearances depending on the position of the section inside the fiber. On longitudinal sections, when the section passes over the cylinder-axis, it remains visible in its entirety, and the dumbbells which connect it to the external network of myelin give the appearance shown in the photographs of the figs. 1, 2, and 3, pl. 31; figures 1, 2, 3, 4, plate 32; figure 1, plate 33.

When the section passes, on the contrary, below the cylinder-axis, we see only the posterior half of the fiber showing the network of the intermediate zone of the myelin sheath which, in this case, takes on the particular appearance shown by the photographs of figures 4 and 5, plate 31.

In the simplest case, which is that of the photographs of figures 1 and 2, plate 31, the cylinder-axis is connected to the membrane or external network by a single row of radiating dumbbells, each of which has its central ball articulated with the network of dumbbells of the internal membrane. The dumbbells clearly act as flying buttresses holding the cylinder-axis in the middle of the nerve fiber.

In other cases, the cylinder-axis is joined to the external wall of the fiber by 2 or 3 rows of dumbbells; those which start from the cylinder-axis have their external ball articulated with that of 2 or 3 other dumbbells which will themselves articulate, either directly to the external membrane, or with a third row of dumbbells.

Finally, a third very frequent case arises; it is that where the cylinder-axis, having a very irregular shape, its distance from the external membrane is also very variable. In this case, there may be a single row of dumbbells in a narrow sector, and two rows or even three in wider ones. These variations will be seen by examining with a magnifying glass the transverse sections of myelinated nerve fibers of the rabbit sciatic in figures 2 and 4, plate 32; one will see there, in addition to clear shapes of dumbbell organelles, the devices that I have just indicated. They will also be seen at several points in figure 1, plate 32, which is a photograph of a longitudinal section of the rabbit sciatic fixed by formalin. Figure 3 of the same plate is the photograph of a transverse section of the medullary root of a spinal ganglion of cariacou (*Odocoileus virginianus*) (sublimated formalin fixation, picric acid). The nerve fibers seen in this section are significantly altered by the fixative, but the intermediate network of the myelin sheath and the shape of the dumbbells are quite clearly visible.

Figure 4, plate 31, is a photograph of a fiber whose intermediate network is visible only in the posterior third, the section having passed behind the axis-cylinder (**Bouin**, hema-

toxylin). In this figure, the edges of the meshes, that is, the dumbbells, are coated, either by undissolved myelin or by a substance coagulated by the fixative; this figure represents the appearance of the branched tracts described by **Nageotte;** the fiber is notably altered, the dumbbells are barely colored.

The neighboring fiber clearly shows (Fig. 3, Pl. 31, **Bouin**, hematoxylin) the intermediate dumbbell network (see under a magnifying glass). Here the fiber is cut obliquely; the section passes downwards through the posterior part of the cylinder-axis to pass tangentially to the upper quarter and in front of the fiber, the region where the external or **Lanterman** network is seen.

Finally, the reader will find in Plate 33 some beautiful elements of studies of the neurokeratin network; these figures are photographs of a section of a rabbit sciatica taken 20 hours after death (48 in the case of Figure 3, Pl. 31) and fixed for 36 hours with **Bouin's** mixture. These sections are then stained with hematoxylin.

This sciatic nerve had been prepared to demonstrate the inaccuracy of the claims of a contradictor who asserted first that mitochondria are destroyed and disappear from organs immediately after death, and second that fixative fluids containing acetic acid, such as **Bouin's** fluid, for example, radically destroy them. The result of the study carried out on this subject was:

1. That the mitochondria of a rabbit sciatic nerve are still intact 48 hours after death;

2. That fixative fluids containing acetic acid alter mitochondria more quickly than other fixatives, but that this fact does not hinder their study in certain tissues, notably in nerves, since they are clearly visible after fixation for 24 hours in **Bouin's** fluid (fig. 3, pl. 31) of a sciatic nerve taken 48 hours after death and after 36 hours of fixation in the same fluid of a rabbit sciatic nerve taken 20 hours after death (fig.1, 2, 3, 4, pl. 33).

The nerve fibers shown in these photographs are severely altered by the fixative, but this alteration in no way prevents the study of the constitution of the myelin sheath. In fibers a (fig. 1) and d (fig. 2), the shape of dumbbells will be clearly seen, and in fibers g, c (fig. 1), d, e (fig. 2), h, i (fig. 3), j (fig. 4), we can clearly distinguish the constitution of the intermediate network between the internal and external sheaths of the neurokératin network.

The dumbbells of the network have only taken on a weak staining by hematoxylin due to the action of the acetic acid in the fixative. Furthermore, in the four figures, the dumbbells of the network are coated with a coagulum or myelin, which has the effect of making people believe that the sides of the meshes, or what remains of them, are tracts in which a mitochondrion is included or simple tracts having a real existence when this

one has been destroyed there or no longer stains but in reality the tract is strictly an artefact; only the dumbbell that it contains really exists. Summarizing what we have just observed, we will conclude that the middle part of the intermediate neurokeratin network between the internal and external sheaths is made up of:

1. Or by a single row of dumbbells, each of which has its internal ball articulated with those of the dumbbells of the internal sheath against the cylinder-axis, and its external ball articulated with that of the dumbbells of the external network, or **Lanterman** network;

2. Or by a network formed, depending on its extent, by two, three, or even four rows of dumbbells articulated together by their ball; the central dumbbell has its central ball articulated to the cylinder-axis and its external ball articulated to the central balls of 2 or 3 other divergent dumbbells, whose external balls articulate either to the balls of the dumbbells of the external network (**Lanterman** network), or with a new row of dumbbells which will articulate outside with this external network;

3. The meshes of the network thus formed are occupied by the myelin in the fresh and normal nerve.

INNER SHEATH OF THE NEUROKERATIN NETWORK

This sheath is quite difficult to see because it is often hidden by the network of the middle zone of the myelin sheath and above all, I believe, because, being made up of a very fine and delicate network, fixatives easily destroy it.

The photographs in Figures 4, 5, and 6, Plate 34, show this sheath extremely clearly, especially Figure 4, in which the very fine network has been well preserved. We also see its fine network applied against the short portions of the axis-cylinder in Fig. 3 of the same plate, particularly in the central fiber.

In all these figures, it is faithfully applied to the axis-cylinder, whose shape it follows in its expansions or contractions.

From the fine network of this sheath, the conical membranes that constitute the **Schmidt -Lanterman** notches stand out. Examples of these can be found in Figures 4, 5, and 6, which already establish that they are also formed by a network and not by spiral filaments (**Rezzonico** apparatus). Figure 6, plate 34, shows:

1. In the left fiber, a piece of preserved axis-cylinder, on which we notice, at its lower part, a piece of the conical membrane of an incisure and, exactly in the middle of the fiber's height, the remains of the inner sheath of the destroyed neurokeratin network;

2. In the central fiber, a pulled, deformed axis-cylinder which shows, towards the middle of the fiber, a small dilation and, below this, covered by the inner sheath, a narrowed portion connected to the outer sheath by transverse dumbbells. The same device will be seen in figures 1 and 8, plate 36;

3. In the lower third of the height of the very altered right fiber, some traces of the inner sheath persist with preservation, on the left, of four dumbbells which connected it to the outer sheath.

* * *

Schmidt-Lanterman Notches and Conical Membranes Rezzonico Apparatus

Schmidt-Lanterman notches consist of infundibuliform or conical membranes whose apex is inserted into the inner sheath of the neurokeratin network and the base into the outer sheath of the same network.

Rezzonico described a continuous spiral filament in these membranes that would act as a supporting apparatus. According to **Nagcotte**, there would exist not just one, but a series of distinct spiral filaments and, in addition, a large number of granulations.

The incisures are visible in Fig. 1 of Plate 29, which is a photograph of **Lanterman's** original drawing showing the network that bears his name.

Here is what **Nageotte** writes about **Rezzonico's** apparatus:

*I was able to demonstrate this apparatus perfectly by staining with acid fuchsin pieces fixed to the **J. de Laguesse** liquid (fig. 24). This fact is important, because this structure had only been stained by silver impregnations, and we know how suspect this technique is when it is not corroborated by other methods of preparation. It is true that **Golgi, Rezzonico, Catani** claim to have stained the filaments in question by several usual techniques and by osmic acid, but as their descriptions and their figures show, they probably only saw the cleavages of the myelin which form so easily at the level of the incisures and which can very well simulate the real **Rezzonico** apparatus.*

The photographs contained in plates 34, 35, and 36 demonstrate that **Rezzonico's** apparatus does not exist and that the granulations reported by **Nageotte** are the balls of the dumbbells that constitute the conical membranes, dumbbells that escaped the investigation of these two authors.

Figures 1 to 6, plate 34; 4, 5, 6, plate 35; 1, 2, 8, 9, 10, plate 36, are photographs of sections of the rabbit sciatica fixed with formalin and stained with ferric hematoxylin. From the first examination of these figures, we realize that the fixative has altered the nerve

fibers. But this alteration is not disadvantageous for the examination, quite the contrary Proof of this is provided, for example, by examining figures 4, 5, 6 of plate 34 and 4, 5, 6 of plate 35, which show in a very clear and apparent way the constitution of the conical membranes; in these figures, the altering action of the fixative has not been strong enough to completely suppress the organization of the fiber; enough remains to establish this organization with certainty and, on the other hand, the treatment with alcohol and toluene for inclusion in paraffin has made the internal organization of the fiber completely visible.

The reader who examines figures 2 and 9 of plate 36 and figure 3 of plate 34 with a magnifying glass will see that these conical membranes are constituted by a fine network with polygonal meshes and that these meshes are formed by dumbbell organelles.

Referring then to Figure 4, Plate 35, he will see at the right margin and in the upper third, an altered and deformed conical membrane, but still clearly showing its network and several of the dumbbells that constitute it.

In Figure 4 of this Plate 34, the left fiber, which has already given us a fine demonstration of the internal sheath of the neurokeratin network applied against the cylinder-axis, also shows us in the upper third a conical membrane of which we only see the section; this membrane starts very clearly from the network of the internal sheath to merge at the periphery of the fiber with the external sheath or **Lanterman** network. I have shown previously, by studying the **Lanterman** network, that its appearance is modified at the points where the conical membranes join it, points very distinct by the fineness of the network at this level. Plate 30 contains fine demonstrations of this fact in Figures 1, 3, 4, 5.

Let us also note that in none of the conical membranes photographed in plates 34, 35 and 36 do we notice the spiral filament(s) described by **Rezzonico** and **Nageotte**. I can therefore state categorically that these spiral filaments do not exist; indeed, it is impossible that my preparations and photographs which showed the fine and delicate network of the internal sheath (fig. 4, pl. 34) and the fine network of the conical membranes which these two authors did not see, would not have shown the spiral filaments if they really existed. But if they do not exist, some of my photographs show in the conical membranes certain dumbbells whose direction is transverse to the axis of the fiber and which are most certainly what **Rezzonico** and **Nageotte** took for spiral filaments.

Figures 5 and 6 of Plate 35, which are two photographs at two different magnifications (1,100 and 550) of the same nerve fiber, show four successive conical membranes which, although altered by the fixative (formalin), are well enough preserved to show their structure. The network and the dumbbells that constitute it are clearly visible;

moreover, more or less regular longitudinal tracts formed by dumbbells articulated together end to end by their balls are noted.

These longitudinal tracts are joined together and also to the cylinder-axis by transverse dumbbells which, in a superficial examination, could at most be confused with filaments, especially if the preparation examined is not very clear and shows nerve fibers too altered by the fixatives; but a careful examination of a fairly well-preserved fiber like the one represented in figure 5, plate 35, clearly shows that there are only distinct transverse dumbbells, articulating by their balls with longitudinal dumbbells which, placed end to end, give the appearance of a tract; the transverse dumbbells, on the other hand, are not articulated in such a way as to give the appearance of a tract spiral.

Let us note, once again, that this presentation shows the absolute necessity of photographing what we see and describe, and not of drawing it, because the drawing cannot contain what has escaped the observer's eye and contains, on the other hand, all the inaccuracies of appreciation and observation.

I still have to point out certain facts relating to the number and direction of the conical membranes. Without any discernible reason, some myelinated nerve fibers lack conical membranes over a fairly long length, while in others they are very abundant and very close together, as shown in Figures 2 and 9 of Plate 36 and Figure 6 of Plate 35.

A whole series of these conical membranes may be parallel, in such a way that the cones would fit into each other if they were pushed against each other. In other cases, they are directed in opposite directions in two neighboring fibers.

Frequently, two successive membranes are directed in opposite directions and, with the **Lanterman** network they have joined, circumscribe a more or less long cylindrical space that is conical at both ends.

Examples of these formations are seen in Figures. 2, 9, and 10 of Plate 36: in the nerve fiber on the right side of Fig. 2, in the right-hand nerve fiber of Figure. 9, and in three fibers of Figure 10.

In other cases, two conical membranes flare out abruptly and join at their bases. An example of the figure thus formed is seen in the upper left corner of Figure 3, Plate 34.

Figures 3, 4, 5, 6, and 7 of Plate 36 are photographs of filaments of an undetermined hyphomycete species of the genus *Mucor*, showing rounded (Fig. 6), oval, or cylindrical formations similar to those just described. Recall that, in the first volume of this work, it was shown that *Mucor* filaments possess an axial cylinder and constrictions so identical to those of myelinated fibers that, in some cases, it is very difficult to distinguish them. Recall also that in the second volume, it was demonstrated by photographic documents

that the axial cylinder of *Mucor* filaments has a fibrillar constitution; these fibrils are made up of dumbbells, and the space between the axial cylinder and the outer membrane of the fiber is occupied by a network of dumbbells connecting these two parts.

This identity of constitution between myelin nerve fibers and mucor filaments is not surprising, since the demonstrations presented in this volume show us the absolute, perfect identity of the constitution of animal cells and plant cells.

* * *

Ranvier constrictions

The photographs contained in plates 34, 35 and 36 do not provide new documents on the constitution and significance of the constrictions of the myelin nerve fibers; they only allow us to clarify the significance of the formations called by **Nageotte** "*double spiny bracelets*." **Nageotte** gave an explanation of the latter which does not correspond to reality; according to him, each spiny crest corresponds to the insertion of one of the myelin layers; he states that by treating a myelin nerve fiber with acetic bichromate, the myelin disintegrates into a certain number of layers which would correspond exactly to the number of spiny crests of each bracelet. This explanation is inaccurate because the myelin sheath is not composed of separate sheets, and acetic dichromate does not divide it into sheets at all.

The anatomical constitution of the myelin sheath, which we have just explained, clearly opposes this explanation. Myelin is a soft substance that fills the meshes of the neurokeratin network and, because of its consistency and the constitution of the network, cannot be arranged into sheets.

The concentric circles that **Nageotte** drew in Figure 21 of his book (d) are never seen in cross-sections of myelinated nerve fibers, and do not exist in his photographic reproduction of a cross-section in Figure 22 of that same book.

On page 742 of his treatise on histology, **Ranvier** describes a lamination of myelin, but he limited himself to a brief observation and did not provide any figures or demonstrations on this subject.

One only has to read this description to realize that what he observed is only an artifact, which is, moreover, inconsistent, since not all the nerve fibers in a preparation show this cleavage, which, moreover, according to **Ranvier,** is reduced to a division of the myelin into two layers and not to ten or even more, as shown in figure 15 of **Nageotte's** book. The latter depicted, in his diagram of the myelin fiber (fig. 14), a sheath made up of separate layers and, on page 221, he states this:

What has been taken for filaments is nothing other than the optical section of excessively thin lamellae and I have assured myself that this aspect reveals the cleavage of the myelin sheath into very numerous layers. As this accident occurs first of all at the level of the ***Schmidt-Lanterman*** *notches, there appear there often very elegant figures which clearly show the regularly laminated structure of the myelin sheath, figures 12 B and 15.*

What **Nageotte** represented in Figure 15 does not correspond to a lamination of the myelin, but to the dumbbells that constitute the membrane of the incisures and to other dumbbells connecting this membrane to the networks of the internal or external sheaths.

Moreover, if this arrangement of incisures corresponded to a lamination of the myelin, it is not clear why it would be located at this point only and would not exist along the entire length of the sheath. In summary, the alleged cleavage of the myelin is an artifact with regard to **Ranvier's** observation and an error of interpretation with regard to **Nageotte's** observations.

The crests of the spiny bracelets therefore do not respond to the insertion of non-existent sheets of myelin. The spines of these crests are what remains of the dumbbells of the neurokeratin network of the myelin sheath at its point of insertion on the cylinder-axis at the level of the constriction.
Let us specify that *it is not the rods of these dumbbells which are inserted on the cylinder-axis, but their balls*; they are therefore circular rows of round granulations, each surmounted by a spine that **Nageotte** should have drawn and not just spines.

Moreover, he indicates on page 238 of his book that: *In the central nervous system, the spiny bracelets have a special appearance; they are made up of a double ring of large rounded granulations*. In this case, **Nageotte** saw only the balls of the dumbbells, while in the other, he saw only the remains of their rods, which he calls spines, and in neither case did he suspect a relationship between the spines and the balls, that is, the existence of the dumbbell organelles.

It is therefore the insertion of the dumbbells of the neurokeratin network onto the cylinder-axis at the level of the constriction that **Nageotte** designated as the spiny bracelet.

We can also see similar spiny bracelets, but with a single row of dumbbells, at the point of insertion of the conical membranes onto the cylinder-axis when they are very slightly inclined and almost perpendicular to it.

Figures 1 and 2 of Plate 34, which are photographs at magnifications of 100 and 550 of the same myelinated fiber of the rabbit sciatic nerve, show in their lower third a spiny bracelet or rather a dumbbell bracelet of this kind.

There can therefore be no doubt that the spiny bracelets are made up of the dumbbells of the neurokeratin network, the balls of which are coated with myelin, which prevents them from being distinguished, and of which only a fragment of the rod is visible, still adhering to the ball, the rest being destroyed by the fixative.

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Fibrillar Structure of the Cylinder-Axis and Structure of the Neurofibrils

This well-known fibrillar structure is clearly marked in the photographs in Figs. 1, 2, and 3 of Plate 34 taken from sections of rabbit sciatica stained with ferric hematoxylin.

By carefully examining the fibrils of the cylinder-axis in these three figures with a magnifying glass, it is possible, although with difficulty, to distinguish some of the dumbbells that constitute them, at least on the photographic prints of the original images; their rods stain with difficulty, while the rods of the dumbbells of the neurokeratin network stain very strongly. As for the balls of the dumbbells of the neurofibrils, they hardly stain at all and can only be distinguished by their vivid refractive index or by their outline, which is sometimes quite apparent.

Ramon y Cajal, Maccabruni, and **Nagcotte** have seen that the impregnation of neurofibrils by methylene blue or silver salts does not occur, at first, in a uniform manner, but only in places, and that this results, for each fibril, in the appearance of small rods arranged in rows and separated from each other by uncolored spaces. This fact is visible in the left part of Figure 2, Plate 37, borrowed (Fig. 30) from **Nagcotte's** book (27). Subsequently, the coloration becomes uniform.

The discontinuity of coloration of neurofibrils is normal and usual; their continuous appearance is abnormal and results from an excess of coloration. **Nageotte** writes on this subject (27, p. 244):

Is this a real discontinuity or an effect of the technique highlighting less easily stain- able points in the continuity of the neurofibrils? It would be very difficult to say. But this uncertainty does not exist when studying healthy nerves, handled with care, and especially when one can highlight, in a single cylinder-axis, a whole series of intermediaries between the regions where the neurofibrils are perfectly continuous, those where they are provided with paler points separating darker spaces and finally those where one no longer sees anything but a crowd of perfectly stained rods, but apparently isolated from one another. Each of the stained segments perfectly resembles, as ***Maccabruni*** *saw, the mitochondria of the cylinder-axis, which can be stained separately by several techniques and which can also be seen in the fresh state, thanks to their high refractive power. But these true mitochondria are, in adulthood, very few in number relative to neurofibrils and their chemical constitution is certainly very different from that of the latter.*

Thus, none of the authors cited have seen that the discontinuity of the neurofibrils is due to the fact that each of them is made up of a series of dumbbells placed end to end, in rows and articulated together by their balls.

Moreover, these authors have refused to admit anything in common between the mitochondria of the axis-cylinder and the colored rods of the neurofibrils; but the assertion that they can be distinguished, either by certain techniques or by a vital examination, remains to be demonstrated by precise documents such as photographs. It has been proven previously, for both animal and plant cells, that it is the dumbbells constituting the cytoplasmic network that have been taken for mitochondria, supposedly free and independent elements, but which have no real existence; only the dumbbell organelles of the cytoplasmic network exist. Likewise, only the dumbbells of the neurofibrils exist in the axis-cylinder.

The demonstration of the existence in the axis-cylinder, in addition to the neurofibrils, of mitochondria in the form of colored rods identical to the rods of the dumbbells will have to include proof that these are not, in reality, the dumbbells of the neurofibrils. The demonstrations made so far in this book establish that the rod-shaped mitochondria (chondriocontes) are dumbbells whose balls are invisible or detached, and that ***the dumbbells are never isolated***; they are always articulated with others to form either a network or a filament. From these facts we must conclude that the existence in the axis-cylinder of rare, that is to say isolated, mitochondria is impossible. There can only exist rods articulated into filaments which, certainly, are the nerve fibrils. Finally, other facts will demonstrate that the neurofibrils are dumbbell filaments.

The confusion of neurofibrillary dumbbells with so-called rod-shaped mitochondria (chondrioconts) has been made not only for the neurofibrils of the spindle-cylinder, but also for the neurofibrils of nerve cells.

Thus, in Figure 2 of his book, representing a motor nerve cell from the rabbit spinal cord, prepared by **Altmann's** method, **Nageotte** has depicted rods that the legend of the figure describes as mitochondria and which are clearly the rods of the dumbbells of the neurofibrils; some of these rods prove by their length that they are fragments of neurofibrils composed of several dumbbells placed end to end; if the coloring is not discontinuous in these long rods or filaments, it is because the coloring (saturated sol. of acid Fuchsin) overcolors them and also because the drawing, always inexact, has perhaps not marked less colored points that could have existed. In any case, the fact that, for each side of the cell, the rods are all placed in the constant direction of the neurofibrils constitutes further proof that they are the very elements that constitute the latter. It could be argued that this direction is due to the fact that the rods are housed in the interfibrillar spaces and, therefore, necessarily parallel to the neurofibrils.

But proof that this is not the case is provided by plate 37 bis (figs. 1 and 4) representing multipolar nerve cells of the cat's spinal cord prepared by silver impregnation. Their extensions are formed by bundles of neurofibrils with a clearly discontinuous appearance consisting of rods placed end to end separated by less colored spaces; some of them, which can be followed inside the cells themselves, are also shown to be made up of the same rods as inside the extensions; some of these dumbbells are quite clearly visible with their balls, at least in my original photographic prints.

In a fairly recent work (1937), **Lévi** and **Meyer** (15) investigated whether neurofibrils already exist in nerve cells from the first days of embryo formation. They conducted this study on fragments, cultured in vitro, of spinal ganglia taken from chicken embryos from the 10th to the 15th day of their incubation. These examinations were carried out by vital observation and by that of fixed preparations which, the authors say, give exactly similar results. (Fixation by picric sublimate and staining by toluidine blue.) Figure 7, plate 37, is the photographic reproduction of figure E, page 413 of their memoir.

According to the authors, the embryonic cells of the spinal ganglia contain a chromophilic substance (tigroid substance) located mainly at the periphery of very young cells such as the one of which only a small segment is seen at the bottom of the figure; in older cells, such as the two large cells, this substance would be distributed in numerous fine concentric striations around the nucleus.

Examination of the figure shows that these striations are fine rods which, at several points, are placed in the extension of each other, which shows that they belong to the same filament.

The authors conclude from their observations that the neurofibrils are the clear spaces of these cells and that the rods or fine colored concentric striations are made up of tigroid substance distributed in the interfibrillar spaces. But they do not consider these rods, ***which are very numerous***, to be mitochondria, as **Nageotte** did for the cell in Fig. 2 of his book.

The authors' interpretation cannot give rise to any doubt on this subject, according to the final sentence of their conclusions, the literal translation of which is as follows:

Mitochondria, which vitally stain with Janus black and methylene blue, are extremely small and rare. They are interfibrillar, as are the chromophilic elements (designated tigroid).

Both authors therefore made a fundamental error. It is the rods or striations of tigroid material that are the intracellular neurofibrils, and apart from them, there are no other elements in the cells they drew or photographed; these neurofibrils appear as separate striations because these are the rods of their constituent dumbbells, whose balls have

not been stained with Toluidine Blue, and leave an unstained space between the striations. This is the picture shown by the neurofibrils of myelinated fibers at the beginning of their staining with Methylene Blue.

Let us recall here that the observations of **Bethe** and **Ramon y Cajal** have demonstrated that neurofibrils are varicose, and that we ourselves have demonstrated here (p. 77 and pl.16) that the cells of adult spinal ganglia are made up of a network of dumbbells.

Other known facts demonstrate that the neurofibrils contained in the myelinated nerve fibers, as well as the pyramidal cells of the gray matter of the cerebral cortex, are made up of dumbbells placed end to end.

Indeed, **Bethe** reported that the pyramidal cells of the cerebral cortex contain bundles of neurofibrils arranged in plexuses and varicose which, coming from the peripheral stem of the pyramidal cells, go to the cylinder-axis and to the other extensions of the cell, the dendrites. (See: **Ramon y Cajal** (28 bis), *Histology of the Nervous System*, p. 545.) On the other hand, we know that the ultimate branches of the dendrites are made up of only one neurofibril, and that this is varicose until its termination. **Ramon y Cajal** (28 bis) who showed these varicosities of the neurofibrils of the dendrites in figures 359 and 364 of this treatise gave them the oval shape that they actually have but without seeing that this oval shape is due to the fact that each varicosity is formed by two balls, articulated together, of two contiguous dumbbells.

We will see later (p. 94) that these varicosities also exist on the filaments that terminate or start from the ependymal cells, and one of these varicosities, clearly visible in Figure 6, Plate 37 bis (upper left corner of square C8) **(1)**, is clearly made up of two joined dumbbell balls. These varicosities have the same shape and are of the same nature as the dilations that **Guilliermond** observed on the path of the chondrioconts of plant cells and which he took for chloroplasts or amyloplasts in formation; it was demonstrated on this occasion that these dilated chondrioconts are fragments of dumbbells detached from the dislocated cytoplasmic network.

Ramon y Cajal wrote (70, p. 553) about the varicosities of neurofibrils:

*The varicose appearance has been considered by some authors, including **Renaut** of Lyon, to be a normal condition. It is, we believe, a cadaveric alteration, due to the concentration of the cyanophilic substance of the protoplasmic juice into droplets. Sometimes the droplets burst, then the substance they contain spreads into the surrounding area and colors it.*

1) *Identification grid.*

This bursting phenomenon is the well-known one that we have previously noted several times regarding the swelling of chondriosomes and dumbbell balls by fixatives and dyes.

It constitutes further proof of the true constitution of varicosities.

The roughly regular spacing of the varicosities of the dendrite branches already indicates that they are a normal arrangement, as Renaut thought, and not an alteration, as **Ramon y Cajal** believed. Another fact that confirms this conclusion is the constitution of the dendrite spines.

Ramon y Cajal (28 bis, p. 553) indicates that the small spines that cover the ramifications of the terminal bouquet of the peripheral stem of the pyramidal cells are short appendages ending in a dark spherule when the preparation is stained by the **Ehrlich** method; this proves that these short appendages are dumbbells and it can be affirmed that their second ball must be articulated on the dendrites with the two balls of a varicosity and not at another point. I have not verified this fact but it seems absolutely certain because the spine is a dumbbell and because the balls of a dumbbell articulate necessarily with the balls of one or more others and not at other points.

This therefore establishes that the neurofibrils of the dendrites of the pyramidal cells are indeed constituted, including their spines, by dumbbells.

In summary, the constitution of the neurofibrils of the axis-cylinder of myelinated nerve fibers by dumbbell organelles placed end to end and articulated by their balls is given by the following facts:

1. By the fact that the branches of the dendrites and in particular those of the terminal bouquet of the peripheral stem of the pyramidal cells are made up of dumbbell organelles whose articulated balls constitute the varicosities;

2. By the fact that these branches, which are neurofibrils, descend into the pyramidal cells and go directly to the cylinder-axis (**Bethe, Ramon y Cajal**) of which they constitute the neurofibrils;

3. By the fact that these neurofibrils which cross the pyramidal cells are, inside them, provided with varicosities (**Bethe**) like the branches of the dendrites, that is to say formed by dumbbells.

It therefore emerges from these facts that in a preparation, the little or no-colored points of the neurofibrils correspond to the balls of the dumbbells that constitute them.

Ependymal Cells

These cells, discussed above (p. 93), are of particular interest to us because of the constitution of their body and that of the filaments that emanate from them.

They are fusiform and, from each end of the spindle, a filament extends, one short and internal towards the ependymal canal and the other long, towards the external membrane of the spinal cord, into which it is inserted, dividing into several branches, each ending in a knob or granulation. These are indications given in histology treatises and which I have not verified. We will only deal here with the body of the cell and the external filament.

Figures 5 and 6 of Plate 37 are photographs of the ependymal cells of the calf spinal cord, fixed with formalin and stained with ferric hematoxylin. These cells are fusiform and contain 2 to 5 or 6 nuclei or chromatic masses intensely stained with hematoxylin; each mass is mammelonated because it is formed by about 8 to 12 joined granulations which are each one of the balls of a dumbbell. The second ball of this articulates, according to its position, either against the external membrane, or with the balls of the nearest nucleus of which it is a part, thus connecting two chromatic masses. This arrangement is vaguely apparent in two cells at the left corner of Figure 5 and in region G. 14. It is clearly seen on the preparations by varying the point.

As for the two internal and external filaments, they each arise from a granulation located at the internal or external end of the spindle, and which belongs to a dumbbell whose other ball is part of the nearest nucleus.

As for the filament itself, it is also made up of dumbbells placed end to end and articulated by their balls, that is, provided with varicosities identical to those of the neurofibrils that constitute the dendrites and the axis-cylinders of the myelin fibers. These varicosities are made up, like those of the neurofibrils of the dendrites, of two joined dumbbell balls. One of them is visible in the upper left corner of square C. 8.

By following one of these external filaments, we see that it is articulated all along its path with other dumbbells that anastomose it with other filaments.

In summary, ependymal cells are not, in reality, complete cells but only nuclei whose chromatic masses are karyosomes, and they do not possess cytoplasm.

Due to the special constitution of these cells and their filaments, which appear identical to neurofibrils, it appears that their ensemble constitutes not a support apparatus as is

commonly accepted, but a noble and functional element of nervous tissue whose role is unknown.

* * *

The preceding observations and explanations have demonstrated:

1. That the neurokeratin network of the myelin sheath is constituted exclusively by dumbbell organelles;

2. That the constitution of this network is identical to that of the cytoplasmic network of cellular elements;

3. That the neurofibrils which constitute the dendrites and the terminal bouquet of the protoplasmic stem of the pyramidal cells of the cerebral cortex are provided with varicosities, and that they continue to present this characteristic inside the cells where they travel to reach the cylinder-axis which they constitute;

4. That the varicosities are constituted by two joined balls of two of the dumbbell organelles articulated end to end to constitute the neurofibrils;
5. That, consequently, the neurofibrils of the cylinder-axis of the myelin nerve fibers are varicose and constituted like the neurofibrils of the dendrites by dumbbell organelles articulated end to end by their balls; this explains the discontinuous appearance they present when treated with methylene blue, which initially colors the rods of the dumbbells first, the balls having less affinity for this dye.

The neurofibrils of the dendrites and protoplasmic stalk of a large pyramidal cell, for example, penetrate this cell and, according to **Bethe**, confirmed by **Ramon y Cajal**, form a very tight plexus by anastomosing together by short branches.

This plexus occupies the space between the cell nucleus and its external membrane, that is, the space occupied in every cell by the cytoplasmic network.

Now, a portion of the neurofibrils of this plexus penetrates the axis-cylinder, which, therefore, must be considered its extension.

Consequently, the neurokeratin network of the myelin sheath cannot also be an extension of this plexus and the perinuclear space; it can only be considered an extension of the pyramidal cell membrane.

We have seen previously that this neurokeratin network of the myelin sheath of nerve fibers has the same constitution and fulfills, with respect to the axis cylinder, the same mechanical role as the cytoplasmic network of any cell with respect to its nucleus.

This leads us to investigate whether the space between the nucleus and the wall of the nerve cell is its cytoplasm and, if it is not, what part could constitute it and give rise to the neurokeratin network of the myelin sheath that arises very close to the origin of the axis cylinder.

* * *

Location and Constitution of the Cytoplasm of Nerve Cells

We are thus led to examine whether the **Golgi** network, which would contain the space between the cell membrane and the nucleus of the nerve cell, is the cytoplasmic network and the origin of the myelin sheath. Until now, it has not been sufficiently taken into account that **Golgi** described two networks in nerve cells that were very different in form and location.

Ramon y Cajal (70, pp. 549 and 550) clearly established the necessary distinction between these two formations; he described the intracellular formation usually called the **Golgi** *Apparatus* under the name of *Intraprotoplasmic Canaliculi* of **Golgi-Holmgren**, and a very different formation, exclusively extracellular or pericellular, whose appearance has nothing in common with that of the previous formation, under the name of *Pericellular Network of* **Golgi**.

Ramon y Cajal (28 bis), who studied the pericellular **Golgi** network also highlighted by **Bethe**, describes it as follows:

Its narrow, rounded and very regular meshes extend in a single plane, right against the cell membrane and outside it. Often they stop at the level of the large polar expansions of the neuron and then present a thickening, a sort of very strongly impregnated ridge at this point; other times on the contrary they extend onto the dendrites...

In Figures 357 and 358, we show the appearance of two short-axis cylinder cells in the cat's brain covered with this network. We see that it envelops the body, dendrites, and axon; it descends along this extension to the beginning of the myelin sheath, where it suddenly stops. It is truly strange that ***Ehrlich's*** *method and our special silver formula only impregnate the superficial network of short-axis cylinder cells in the entire cerebral cortex, and more particularly of the neurons that send their large axis cylinder to the plexiform layer.*

Figure 357 by **Ramon y Cajal** (28 bis) is reproduced in Figure 3 of Plate 37 bis of this book.

The reader will surely see for himself upon reading this quotation that **Ramon y Cajal** was thus indicating a fact, the continuity of the pericellular **Golgi** network and the myelin sheath which, without him having suspected it, demonstrated:

1. The existence and location of the cytoplasm of the nerve cell;

2. The origin of the neurokeratin network of the myelin sheath.

This is indeed a network identical to the cytoplasmic network of any cell, a network which, as we indicated previously, always has its meshes and dumbbells arranged on a single plane in the external cell membrane.

The external pericellular **Golgi** network therefore indeed represents the cytoplasm of the nerve cell and the fact of the continuity of the myelin sheath of the axis cylinder with this network, as explained by **Ramon y Cajal**, demonstrates that this sheath and its neurokeratin network are indeed the continuation of a single object: the cytoplasm of the nerve cell, that is, the neuron.

It is likely that, in reality, the pericellular network is the external cell membrane itself and that, in all cases, the neurokeratin network of the axon is an extension of it.

As for the **Golgi** apparatus, which **Ramon y Cajal** (70) also studied and which he represented in his figure 356, reproduced in figure 2 of our plate 37 bis, he considers it to be the vacuolar and tubular apparatus, arranged in a network and generally confined to the vicinity of the nucleus, which had already been described by **Retzius, Holmgren,** and others in the cells of the spinal ganglia. He indicates (p. 549) that:

These canaliculi are all the more scattered and numerous as the pyramidal cell is larger; they are particularly accumulated above the nucleus, where they seem to merge into a single canal that occupies the axis of the protoplasmic trunk.

Ramon y Cajal indicates that, with the help of a special formula with reduced silver nitrate, he was able to observe the existence of this tubular apparatus in almost all the cells of the brain.

We ourselves investigated the existence of this tubular apparatus in the cells of the spinal ganglia (p. 77) and it was demonstrated, on this occasion, that these cells do not contain any apparatus or network of canals of the type described by **Holmgren** and **Golgi** and that one only notices a very tight network of dumbbells which form only very small meshes by their agglomeration (fig. 1, 2 and 3, pl. 16 of this book).

The network of canaliculi described by **Holmgren** and **Golgi** appears to respond only to alterations produced by the fixing and coloring liquids since they saw neither the dumbbells which occupy the entire cellular space outside the nucleus, nor the fine meshes which they form by articulating by their balls.

Holmgren saw and described these same canaliculi in the cytoplasm of hedgehog liver cells. However, it has been demonstrated in this book that the cytoplasm of animal and plant tissue cells, and particularly mammalian liver cells, is made up of a network of dumbbells hinged together by their balls to form open polygonal meshes through which the cytoplasmic liquid circulates freely in all directions, the whole of which constitutes a single cavity. Under these conditions, a system of canaliculi is completely useless, pointless, and certainly does not exist. We can therefore conclude, in any case, that this **Golgi** tubular apparatus of **Holmgren**, which certainly does not exist, could, in any case, have no connection with the formation of the neurokeratin network of the myelin sheath of nerve fibers.

This network is indeed, as stated above, the continuation of the pericellular **Golgi** network. Although this origin of the myelin sheath is only apparent for short-axis cylinder cells of the 3rd layer of the gray matter of the hemispheres, the demonstration nonetheless has a completely general scope.

To complete this analysis of the morphological constitution of nerve cells, we must examine how the nucleus is maintained in a fixed position, because, in every cell, this condition is fulfilled by the cytoplasmic network whose central dumbbells have one of their balls attached to the outer wall of the nuclear membrane.

It is certain that connections must exist between the nucleus of pyramidal cells and the cell wall, but we have no indication of them; we can only hypothesize the existence of numerous dumbbells articulating with the plexus of neurofibrils and, on the other hand, either with the outer membrane or with the outer wall of the nuclear membrane.

Real Origin of Neurofibrils

It has just been explained that the neurofibrils that constitute the descending cylinder-axis of a large pyramidal cell come from the terminal bouquet of the protoplasmic rod and that their point of origin is probably the plexiform layer of the gray matter.

But the neurofibrils of the cell's lateral dendrites will get lost on all sides in the amorphous matter of the gray matter of the cerebral cortex.

The new knowledge of the constitution of neurofibrils by a series of dumbbells articulated end to end by their balls, and, on the other hand, the mode of multiplication of dumbbells, considerably facilitates the search for the origin of neurofibrils. It allows us to consider that, in principle, a neurofibril can only come from two sources:

1. Either from a dumbbell of an already existing neurofibril, a dumbbell which, at the level of a varicose vein, laterally forms another which will be the origin of a new neurofibril;

2. Or from a single original granulation which has formed a chain of new dumbbells.

Since a neurofibril always begins or ends with a dumbbell, it therefore has, like the latter, a ball at each of its ends. Indeed, **Ramon y Cajal** (28 bis, pp. 352 and 353), who was able to follow the path of the short collateral branches emitted by the axon of the pyramidal cells of the 3rd layer of the gray matter of the cerebral cortex of the cat, observed that they end in a varicosity, that is, in a ball. We have already seen else- where that the spines of the dendrites always end in a spherule or ball. This leads us to predict that the real origin of neurofibrils must be a dumbbell. We will also demonstrate that the normal or abnormal filamentous elements of the animal organism or developed in in vitro cultures are formed at their point of origin, either by a simple granulation or by a preformed dumbbell.

We will make this demonstration:

1. For connective fibers;
2. For the connective fibers that constitute the swirling formations or horny globes of cancerous tissues;
3. For filamentous elements that are easily generated from the granulations contained in normal blood cells;
4. For the filamentous form of Koch's bacillus.

* * *

1. FORMATION OF CONNECTIVE TISSUE FIBER BUNDLES

The study of the morphological constitution of the lumps which form spontaneously in fibrinogen solutions, even purified by the addition of gelatinous tricalcium phosphate, shows that they are small masses of fibrin in which we note, among other elements, the presence of compact masses of granulations, each of which gives rise to a long, undulating, hyaline filament, identical to the elastic fibres of connective tissue.

Enormous bundles of fibrils are thus formed in these lumps. One of them is shown with the mass of granulations that gives rise to it in Figure 4 of Plate 119 of the first volume of this work and is reproduced in Figure 4 of Plate 37b of this third volume. It is difficult to decide whether these hyaline, poorly staining fibrils (elastic fibers) are made up of a simple, continuous filament or a filament of dumbbells articulated by their balls. Some filaments, with several balls along their path, appear to be made up of dumbbells.

It was shown, on pages 71 and 214 of the first volume, that bundles of connective fibers arise from germinative masses packed with simple granulations that give rise to connective fibers in the formation of scar tissue in wounds.

Figure 3, Plate 37 ter, is a photograph of the framework or framework of the wall of a pulmonary alveolus in the process of destruction. It shows that this framework is made up of dumbbell filaments, as can be seen in regions ***n*** and ***m***.

2. FORMATION OF THE CONNECTIVE FIBERS THAT CONSTITUTE THE WHIRLS OR HORNED GLOBE OF CANCEROUS TISSUE

This subject is covered in Chapter XIV, The Study of Cancer. It will be seen there that *the connective fibers that constitute the whirlpool (vortex) formations are filaments formed by a series of dumbbells articulated end to end by their balls* (fig. 2 and 4, pl. 46) and that they arise from embryonic cells, exactly like the filamentous form of Koch's bacillus, that is to say, from preformed dumbbells.

3. FILAMENTOUS ELEMENTS FORMED UNDER CERTAIN CONDITIONS BY THE GRANULATIONS CONTAINED BY THE BLOOD CELLS

It was demonstrated in the first volume of this work that the blood cells of the guinea pig are formed by a mass of areolar tissue on which pedunculated granules form (fig. 1, pl. 37 ter reproduced from plate 108, first volume). At that time I had neither seen nor understood that the normal globular framework was a network of dumbbells articulated by their balls, these being the pedunculated granulations that I had noticed. The new knowledge presented in this volume now sheds light on this anatomical arrangement as well as the curious fact that I had observed of the formation of long filaments by blood cells subjected to a hemolysis reaction by a sensitizer and a normal serum at a temperature of 37°; these filaments, represented in Figure 2, Plate 37 ter, are formed by dumbbells placed end to end and originate from the multiplication of the dumbbells of the normal globular framework in the process of dislocation.

4. ORIGIN AND DEVELOPMENT OF THE FILAMENTOUS FORM OF KOCH'S BACILUS

The second volume of this work contains the demonstration of the nature and origin of Koch's bacillus. Developing and expanding tuberculous tissue contains a large quantity of small, round cells that are embryonic cells. These, formed by one of the balls of a dumbbell organelle, are made up of a group of these organelles. When these cells have reached their full development, the balls of their dumbbells give rise to:

A) Either long filaments that constitute the framework of tuberculous tissue. These filaments are formed by dumbbells placed end to end, each of which is a Koch's bacillus (see pl. 29, 33, 34, and pp. 74, 79, 80, 81, second volume).

B) Either to skeins of dumbbell filaments when embryonic cells fall into a cavern or are embedded in the caseous matter of a softened tubercle. Each of these filaments is formed by a series of Koch's bacilli dumbbells. Figure 6, plate 37b of this volume shows one of these skeins arising from an embryonic cell germinating in the caseous matter of a very small cavern. It is these rods of the dumbbells that have taken on the color, their balls being generally uncolored.

Figure 8, plate 37 ter, shows, in this same caseous matter, two embryonic cells joined together giving rise, with great clarity, to two dumbbell filaments ***f, c,*** each clearly starting from a very visible dumbbell ball; this is a very clear demonstration of the mode of development and the origin of the filamentous form of Koch's bacillus. Figure 5 (pl. 37 ter) shows an embryonic cell giving rise to several dumbbell filaments.

One, very short and comprising only 3 or 4 dumbbells, clearly starts from a dumbbell ball located on the left side of square E 7.

C) Or isolated bacilli. Figure 7 (plate 37 ter) shows a group of embryonic cells, one of which (square D 4) has formed a group of Koch's bacilli arranged like pins stuck on a ball of wool. Other isolated bacilli arise from an unstained, refractile dumbbell ball (B 5, K 5). The four figures 5, 6, 7, and 8, plate 37 ter, are photographs of the same preparation of caseous material from a very small cavern. Finally, Fig. 5, plate 17, shows an embryonic cell emitting several filaments of Koch's bacilli, one of which, at the very beginning of its development, has its formative ball located on the left side of square E 7 of the marking grid.

All the examples just cited therefore demonstrate that the filamentous elements arise either from an isolated granulation or from one of the balls of a dumbbell. Since the dumbbell is, by its form, an element that is already evolved and much less simple than the granulation, it is likely that it itself originates from the latter.

CONCLUSIONS

1. The myelin sheath is made up of a network of dumbbells articulated together by their balls, a network whose free spaces communicate freely with each other and are filled with myelin. The dumbbells of the external and internal parts of this sheath are arranged along a curved surface. The dumbbells of the middle layer articulate by their balls with those of the external and internal layers in a direction perpendicular or slightly oblique to the latter;

2. The myelin sheath does not have a laminated structure;

3. The constrictions of **Ranvier** correspond to the direct insertion on the cylinder-axis of the balls of the dumbbells of the outer sheath. It is these dumbbells which, partly destroyed by the fixative, constitute the spiny collars described by **Nageotte**;

4. The **Schmidt-Lanterman** incisures are the conical membranes constituted exclusively by dumbbells and which are inserted, on the one hand, on the inner layer and on the other hand, on the outer layer of the myelin sheath;

5. The Rezzonico apparatus formed by spiral fibers does not exist;

6. The cylinder-axis of the myelinated nerve fibers is constituted by neurofibrils identical to those of the nerve centers; they are formed by dumbbells assembled end to end and articulated by their balls which are their varicosities;

7. The cytoplasm of nerve cells is not the space between the nucleus and the mem brane; it is the external polygonal network of Golgi. The network of dumbbells of the myelin sheath is the continuation at the periphery of the cylinder-axis of the nerve fibers;

8. The ependymal cells of the spinal cord are noble elements of the nerve centers.

* * * * *

CHAPTER III

STUDY OF THE NUCLEUS OF ANIMAL CELLS AT REST

When, in a cell, the cytoplasm, which appears more fragile than the nucleus, is fairly well preserved, we can be sure that the latter has a minimum of alteration; but let us say immediately that, despite this, it is always more or less altered by the fixative, a fact proven by the drawings of a considerable number of observers, examples of which can be found in the plates which follow.

It is obvious that, when the cytoplasm is profoundly altered (we have seen previously that this is most often the case), it is impossible for the normal elements of the nucleus not to have undergone a certain degree of alteration; therefore, for studies concerning the morphology of the nucleus, it is appropriate to carry out observations only on cells whose cytoplasm is apparently well preserved, which is a guarantee that the nucleus is even better preserved, since its elements appear more resistant than those of the cytoplasm.

It is appropriate, in publications, to inform the reader by showing the state of preservation or alteration of the cell by means of a photograph of the cytoplasm.

We have seen previously that the fixing fluid that appears to determine the least alteration of the cell is **Locke's** fluid (animals) or **Ringer's** fluid (plants) formalinized in a variable proportion depending on the case (5 to 12%).

I have not made special preparations for the study of the nucleus, except for research relating to karyokinesis; I have mainly sought to study the structure of the latter in preparations where the cytoplasm was best preserved.

It should first be specified that the current study aims exclusively at the structure of the resting nucleus of adult somatic cells. The constituent elements of such a nucleus have nothing in common with the elements of the nucleus during the various phases of karyokinesis.

There are a number of interesting nuclei in the plates of this work, apart from those gathered in plate 40. In this plate, we will find: in figures 1 to 15, nuclei of guinea pig liver cells; in figures 16 to 23, nuclei of cancer cells (uterine epithelomia); in figure 24, a nucleus of an epithelial cell from the rabbit kidney; in figures 25, 26, nuclei of cells from the cerebral cortex of the rabbit. At the bottom of this plate are reproduced three drawings showing the constitution of the nucleus:

1. Of a nerve cell from the cerebral cortex of the rabbit (fig. 27);

2. Of a cell from the liver of the guinea pig (fig. 28). This is the enlarged reproduction of the upper nucleus of figure 4;

3. Of a cancer cell, cell of figures 20, 21, 22, photographed at three different focuses (fig. 29).

The reader will immediately notice by examining these three drawings, particularly figs. 27 and 29, that the nucleus is made up of dumbbell-shaped organelles exactly similar to the dumbbell organelles that make up the cytoplasm.

In the nuclei of the simplest constitution, such as that of Fig. 27, the dumbbells are all radiating, having a ball applied to the periphery, against the nuclear membrane while their central ball constitutes, with the central balls of the other dumbbells, the nucleolus in which they seem fused. But, however, one can often distinguish their edge which thus draws a festoon around the nucleolus. Such a constitution is seen in the central nucleus of Fig. 25, reproduced in the drawing of Fig. 27; one sees it also in Figs. 20, 21, 22, representing the same cell seen at three different focuses and in which there are three large chromatic masses comparable to nucleoli, and formed by the central balls of radiating dumbbells. This cell is drawn in Figure 29 which shows all the dumbbells that can be seen by varying the focus.

Many authors have already reported the star-like shape of certain karyosomes in the nucleus. This star-like shape is due to the rods of the dumbbells, the union of whose balls constitutes the karyosome, because these chromatic masses are always, without exception, constituted by the union of several balls of dumbbell organelles. A clear example of this is found in the nucleus of the rabbit renal epithelium cell in Figure 24. This nucleus contains 6 or 7 large karyosomes around which the rods of the dumbbells can be seen radiating.

When only very short rods remain around the star-shaped karyosomes, it is because the radiating dumbbells that constitute these karyosomes have had their rods altered, and only a very short fragment remains, adhering to them.

When two large karyosomes are neighbors, we notice that they are connected to each other by the rods of several of the dumbbells that constitute them; the rods of the other dumbbells of each of these chromatic masses radiate regularly around them.

Nuclei with a linin network are seen in figures 6, 7, 10, 12, 26. By examining them with a magnifying glass, we will see dumbbells very clearly, and we will be able to realize that the filaments constituting the meshes of the network are dumbbell rods. If, in the well-

formed linin network, the granulations are always found at the points of intersection of the filaments, that is to say at the angles of the meshes, as we see for example in the nucleus of the cells of figures 1 and 2 of plate 41, it is because each side of the meshes is constituted by a dumbbell rod whose balls are placed at the apexes of the angles of these meshes, these being at the same time the points of articulation of the balls between them. We can thus see 2 to 4 balls or more attached together at the angles of the meshes. As in the cytoplasm, these balls are not free; they are constituent parts of the dumbbells and remain immobile with them in the nuclear network. The constitution of the linin network is therefore exactly similar to that of the cytoplasmic network.

* * *

We now have to examine the relationships that exist between the dumbbells of the cytoplasm and the balls of the nuclear dumbbells that are located against the nuclear membrane.

By examining the plates of this work under a magnifying glass (particularly plates 17 to 21), we will see that the central balls of the central dumbbells of the cytoplasm are applied against the outer face of the nuclear membrane, the external balls of the nuclear dumbbells being pressed against its inner face.

Plates 17 to 21, examined under a magnifying glass, show that the rods of the central cytoplasmic dumbbells that articulate with the nucleus have the direction of the extension of the latter's radius.

The central balls of the central cytoplasmic dumbbells are therefore separated from the external balls of the nuclear dumbbells only by the thinness of the nuclear membrane.

This arrangement ensures perfect stability for the nucleus and prevents its movement within the cell.

* * *

The dumbbell shape has been seen in the cytoplasm by various observers and considered to be a stage of division of granular chondriosomes resulting in the formation of two distinct, independent round chondriosomes. They interpreted this shape this way because they were unaware that the dumbbell is the universal formative organelle of living beings and that it is not a passing form, but a definitive form.

The dumbbell always comes from an element of similar shape, from another dumb- bell. Its formation can occur by two different processes:

1. One of the balls of the formative dumbbell would emit, by budding or by division, a new ball which, in turn, would divide into two parts which, separating, would remain connected to each other by a rod stretched by their separation;

2. Or one of the balls of the dumbbell of the formative dumbbell would first emit a first ball and this one a filament which would be the rod of the new dumbbell; at the end of this rod, a new ball would be formed whose substance would be provided by the previous one.

As for a longitudinal division, analogous to those of the chromosomes of the nucleus during karyokinesis, it is certainly not the process of formation or multiplication of the dumbbells of the cytoplasm or of the nucleus of the somatic cell in the process of growth. The study of the embryonic cells of the tuberculous tissue makes it possible to elucidate this point and to establish that only one of the two processes indicated above intervenes.

It has indeed been demonstrated in the second volume of this work that the embryonic cells of tuberculous tissue are constituted by a group of dumbbells and that at a certain moment in their development, the balls of these dumbbells emit a rod which moves out of the cell in the direction of the ray and constitutes either an isolated tuberculous bacillus (fig. 3, pl. 45, and fig. 2, ***l, k, j.***, pl. 46, 2nd volume) which is a newly formed dumbbell, or a fairly long filament if this dumbbell itself constitutes new ones which are articulated end to end by their balls (figs. 1 to 8, pl. 49, 2nd volume); figures 1 and 5 of this plate clearly show the formation of such a filament by the embryonic cells and they seem to indicate that the dumbbell is formed by a very fine filament starting from a ball and that its second ball is formed at the end of this filament and after it.

In summary, the dumbbell always comes from a dumbbell, and since the formation of this organelle in the cell is centrifugal, we are thus able to examine the question of the origin of cytoplasmic dumbbells and the role that the nucleus could play in this formation.

Role of Nuclear Dumbbells in the Formation of Cytoplasmic Dumbbells

The nuclear organelle and the cytoplasmic organelle, which are both dumbbells, therefore have an identical shape and are articulated together by their balls; some balls of the cytoplasmic dumbbells appear to be included in the nucleus, at least in part. Under these conditions, are the chromosome dumbbells or prochromosomes of the nucleus of a different nature from that of the dumbbells constituting the cytoplasm?

Nuclear dumbbells are, like cytoplasmic dumbbells, made up of an achromatic material which constitutes their skeleton or framework, a material impregnated with a certain quantity of chromatic substance.

Feulgen's "*nucleal reaction*" indicates that the shaped elements of the nucleus stain strongly red, while those of the cytoplasm stain only faintly pink, generally, but sometimes even a true pink; which seems to indicate that the cytoplasmic dumbbells contain a certain quantity of chromatin identical to the nuclear chromatin. This there- fore means that the properties common to cytoplasmic dumbbells and nuclear dumbbells are not limited to their shape alone.

We will now present new facts which support either a common origin of nuclear dumbbells and cytoplasmic dumbbells, or the formation of the latter by the former. These facts relate to:

1. The increase of the cytoplasmic network in a centrifugal direction;

2. The mode of formation of giant cells of tuberculous tissue;

3. The germination of embryonic cells of tuberculous tissue and the formation of Koch's bacillus and its filamentous form by these cells, the former giving rise to the latter.

1. GROWTH OF THE CYTOPLASMIC NETWORK IN THE CENTRIFUGAL DIRECTION

We have seen previously, in studying the constitution of the cells of the terminal meristem of the terminal buds of the stem or the radicle in plants, that the interior space of these cells is occupied almost entirely at their origin by the nucleus which is very voluminous, the cytoplasm being very reduced and constituted by a single row of short radiating dumbbells (pl. 3 and 4).

As we move away from the terminal bud, the cells are increasingly larger, and it is only their cytoplasm that increases, first by elongation of the rods of the first row of dumbbells, then by the formation of a second row forming a network of meshes, then a third row, this multiplication taking place by budding or division of the balls of the dumbbells. It is therefore exclusively by the periphery of the cell that this cytoplasm increases; in these conditions, we can think that its first crown of dumbbells could have been formed, due to this centrifugal increase, by the periphery of the nucleus, that is to say by the multiplication, towards the outside, of the dumbbells of the latter.

2. MODE OF FORMATION OF GIANT CELLS IN TUBERCULAR TISSUE AND GERMINATION OF EMBRYONIC CELLS

The mode of formation of giant cells in tubercular tissue will give us other arguments in favor of this origin of the first rank of cytoplasmic dumbbells. This mode of formation has been studied and described in the second volume of this work on page 65. We know that giant cells are made up of a peripheral crown of embryonic cells, small round cells which constitute the nuclei. Here, unlike what exists for the normal cell, the nuclei are located at the periphery, and it is the cytoplasm which occupies the center of the giant cell **(1)**.

It has been demonstrated on this subject, in the second volume, that the peripheral embryonic cells, as well as the cytoplasm of the giant cell, are constituted exclusively by dumbbell organelles articulated between them by their balls (pl. 21, 22, and fig. 3, pl. 24) and constituting in the giant cell a cytoplasmic network with very fine meshes **(2)**.

It has further been demonstrated that the peripheral nuclei are the site of true germination and emit dumbbell filaments (pl. 23 of the second volume), the anastomosis of which by new dumbbells constitutes the very finely meshed cytoplasmic network of the giant cell (pl. 21, 22, and fig. 3, pl. 24).

These filaments were seen long ago by **Borel,** for example, who considered the peripheral nuclei of the giant cell as lymphatic cells emerging from the vessels and which emit expansions (the filaments indicated above), which go to meet the Koch's bacilli that the giant cell contains in large numbers, to phagocytose them.

In reality, these filaments are intended exclusively for the formation of the cytoplasm of the giant cell, and not for the destruction of the Koch's bacillus since the latter is the dumbbell constituting these filaments.

Here, then, is a case in which a nucleus (for the embryonic cell is indeed a nucleus) forms the cytoplasm of a cell. On the other hand, it has been demonstrated that embryonic cells also emit filaments identical and of the same nature as the previous ones which form the trabeculae (bundles of filaments) of the tuberculous tissue (see plate 41, 2nd volume), the filaments being constituted by dumbbells placed end to end; the embryonic cell is therefore capable of emitting in a centrifugal direction elements similar to those which form it. Here now is a new fact which shows in a striking way the close relationships which can exist between nuclear dumbbells and cytoplasmic dumbbells.

(1) *In relation to each nucleus, the development of the cytoplasm therefore takes place in a centrifugal direction, as in the previous case.*
(2) *A.-Ch. and G. Hollande (2 bis) have indicated around the embryonic cells of tuberculous tissue the presence of a very narrow cytoplasmic zone containing only a few rare granular chondriosomes. I have never been able to detect such a zone, which does not appear in any of the photographs in the plates of the second volume.*

We have seen previously, in numerous plates, that the central dumbbells of the cytoplasm have a radiating direction around the nucleus, this is true in both plant and animal cells.

Plates 45 and 46 of the second volume show the germination of embryonic cells; these give rise to new dumbbells whose rod always has a direction perpendicular to the wall; we see these rods take in certain cells (cell ***a,*** fig. 3, pl. 45, and cell ***a***, fig. 2, pl. 46) the same radiating arrangement around the nucleus as that of the perinuclear dumbbells of the cytoplasm of any cell. We find a very demon- strative example of this in the embryonic cell ***C*** located in the upper part of plate 41 of the second volume.

Comparison of this cell ***C*** with the nucleus located in the upper part of figures 4 and 5 of plate 25 of this third volume, showing the cytoplasm and a nucleus of the adrenal capsules of guinea pigs, is very suggestive. If we consider that this radiating arrangement of the dumbbells is also observed in plant cells (see fig. 2, 3, 4, pl. 3, 3rd volume), we are forced to admit that it is a constant process, and even a law of cell development.

In the case of the embryonic cell, the peripheral radiating dumbbells are indeed formed by it, that is, by a nucleus. This means that, a nucleus being destined to form a cell, that is, to surround itself with cytoplasm, it first emits to form the latter a series of radiating dumbbells whose direc- tion is perpendicular to the peripheral surface of the nucleus. These dumbbells, by multiplying, will constitute the cytoplasmic network.

The embryonic cells of tuberculous tissue do not escape this law; indeed, cell ***a,*** figure 2, plate 46 of the second volume, evolves to transform into an epithelioid cell, and this is formed by a network of dumbbells.

This exposition has shown us, in summary:

1. That the embryonic cells of tuberculous tissue are naked nuclei, of obviously aberrant structure, not yet surrounded by cytoplasm;

2. That these nuclei are formed exclusively by dumbbell organelles exactly similar to those which form the nucleus and cytoplasm of the animal and plant cell;

3. That these nuclei evolve and give rise, at their periphery, to new dumbbells which have exactly the direction of the radius of the cell extended outside it and that the result of this evolution is either the epithelioid cell, or filaments formed by dumbbells (Koch's bacilli), or isolated bacilli when they detach prematurely from the cell;

4. That this radiating arrangement is exactly similar to that of the dumbbells of the cytoplasm around the nucleus of animal and plant cells;

5. That these nuclei germinate to give rise to the cytoplasm of the giant cell;

6. That these facts fit perfectly with the development of the cell which takes place from the center outwards, that is to say from the periphery of the nucleus;

7. That we can deduce from this that the cytoplasm of the cells of the organism of animals and plants is probably formed by the nucleus of these cells, and more precisely by the germination or division of the peripheral balls of the nuclear dumbbells.

In summary, the facts presented so far have given us the following notions:

1. The nucleus of the cell is constituted exclusively by dumbbell organelles, whose rod corresponds to the filaments of the meshes of the linin network and whose balls are the nodal granules of this network or karyosomes, chromosomes, prochromosomes;

2. The cytoplasm is formed by dumbbell organelles articulated by their balls to form the cytoplasmic network, dumbbells exactly similar to the nuclear dumbbells;

3. The central cytoplasmic dumbbells are articulated by their central balls with the nuclear membrane and with the peripheral balls of the nuclear dumbbells;

4. The peripheral balls of the nuclear dumbbells probably form the first series of dumbbells of the cytoplasm which, themselves, will give rise to those which will form the second, then the other rows of meshes of the cytoplasmic network.

Thus appears the morphological identity between nuclear and cytoplasmic dumbbell organelles as well as the probability of their common origin. Other facts that we will present support this common constitution and probably origin between nuclear dumbbells and cytoplasmic dumbbells. Indeed:

Dumbbells are made up, in the cytoplasm and in the nucleus, of a hyaline, achromatic material, which forms their framework, skeleton, or substratum. This material is impregnated with one or more chromatic substances, including the nuclear material called chromatin. This material has the property of staining red in the **Feulgen** nucleal reaction, while the cytoplasm does not stain. In reality, it stains pink, sometimes even quite bright pink, which proves that cytoplasmic dumbbells contain a certain quantity of this nuclear chromatin.

We can therefore ask ourselves, from this point of view, whether it is not only a smaller quantity of chromatin that differentiates cytoplasmic dumbbells from nuclear dumbbells.

On the other hand, staining with hematoxylin also demonstrates the existence in the nucleus and in the cytoplasm of a chromatic material that stains in an identical way, and exists in large quantities in the cytoplasm, but in much larger quantities in the nucleus; in fact, differentiation by any process causes the staining in the cytoplasm to disappear

while the staining of the nucleus still remains vivid. We can therefore say that, from the point of view of the presence of chromatin, there is no absolute criterion that allows us to differentiate the nuclear dumbbell from the cytoplasmic dumbbell.

Other facts support this view; for example: it has been recognized that during kinesis, the nuclear membrane disappears during metaphase and that, from this moment, until the reconstitution of this membrane during telophase, the nuclear medium is in direct contact with the cytoplasmic medium.

However, in the current state of our knowledge, we do not know what exchanges occur between these two environments. These exchanges do exist, however, and it has also been noted by various researchers that, in certain cases, part of the nucleolus is expelled into the cytoplasm during anaphase and before the reconstitution of the nuclear membrane.

Finally, in certain cases, when the nucleolus disappears completely, as many authors have also noted, it is because the substances that constitute it enter into dissolution or colloidal emulsion in the liquid, which implies the passage of these substances into the cytoplasmic medium as well as into the nuclear medium during anaphase.

Now, it is precisely during this period of absence of the nuclear membrane and contact between the nuclear and cytoplasmic environments that the pro-chromosomes or eu-chromocenters, or more precisely, the nuclear dumbbells of the quiescent state, would begin to reconstitute themselves.

Furthermore, current knowledge does not tell us what happened, at this time, to the central and radiating cytoplasmic dumbbells whose balls were pressed against the nuclear membrane. If this hyaline membrane has disappeared, that is, has been liquefied, is it likely that the balls that pressed against them have also been? The drawings of all the authors do not provide us with the slightest information on the behavior of the central part of the cytoplasm during kinesis.

Does the reconstitution of the nuclear membrane at the end of anaphase take place before, at the same time, or after the reconstitution of the first row of cytoplasmic dumbbells that will be established between the two daughter cells?

Are these new dumbbells formed by substances or materials from the cytoplasm or from those of the nucleus? Does the nuclear membrane itself emanate from the cytoplasm or from the nucleus?

It is likely that the reconstruction of the nuclear membrane, that of the normal dumbbells returning to the quiescent state, and that of the new cytoplasmic dumbbells which take place at the same time when there is no longer a boundary between the cytoplasm

and the nucleoplasm, are carried out by substances which are common to them and that, at the very least, the framework of the nuclear dumbbell has the same constitution as that of the nuclear membrane and that of the new cytoplasmic dumbbells.

To accurately appreciate the evolution of the various cellular elements during kinesis, it is necessary to take into account the profound alterations produced by the fixatives and which have prevented us from observing until now:

1. The fact that the euchromocenters or prochromosomes are only one of the balls of a complete dumbbell whose rod has been destroyed by the fixative and has not been seen by the observer;

2. The fact that in the euchromocentric type, the dumbbells all have a radiating arrangement;

3. The fact that in this type the external portion of the nucleolus is made up of the central balls of radiating dumbbells and in the reticulated type by the central balls of a group of dumbbells radiating around it;

4. The cause of the decrease in volume of the nucleolus during prophase, or even its complete disappearance, is due to the fact that it is not only the chromocenters or pro-

chromosomes that contribute to forming the chromosomes, but also the rod and the second ball of the complete dumbbells, ***this one detaching itself from the nucleolus*** of which it constitutes the external part.

This proves that the phenomena of kinesis are far from being known completely and exactly, especially because of the profound modifications produced by the fixing reagents, modifications which require great caution in assessments and conclusions as well as a close control of the facts already known.

* * *

STRUCTURE OF THE PLANT CELL NUCLEUS

At the beginning of this study, I must briefly recall what is currently known about the plant cell nucleus.

It is generally accepted that it has both a reticular and granular structure. The reticulum (linin network), consisting of an achromatic or slightly chromatic substance, is formed of polygonal meshes containing nodal granulations, which are the karyosomes, or chromocenters. In the nucleus, generally in the center, there is one nucleolus, sometimes several. The structure of the nucleolus is completely unknown at present.

Overton (1905) called the chromatic masses seen in the nucleus outside the nucleolus prochromosomes, and **Lundegardh** (1910) called them karyosomes.

The name chromocenter was given to these same masses by **Baccarini** (1908) in nuclei that did not possess a well-developed chromatic network.

Grégoire classifies in a euchromocentric type: "*nuclei where the only parts that are chromatic during interphase, outside the nucleolus, are apparent peripheral corpuscles, lodged against the nuclear periphery and whose number may be equal to, or sometimes less than, the specific number of chromosomes, but never exceeds it.*"

He calls these peripheral granules euchromocenters, which, moreover, correspond to **Overton's** pro-chromosomes. Some reserve the name pro-chromosomes for the chromatic masses of non-reticulated nuclei, and chromocenters for the nodal chromatic masses of reticulated nuclei.

I am only pointing out the discussions that have taken place among observers on prochromosomes, chromocenters, and euchromocenters and on the distinctions to be made between them, mainly because these are simply different names for the same object. Some authors believe that euchromocenters or prochromosomes persist without apparent change during kinesis and until interphase. Others, on the contrary, believe that each euchromocenter or prochromosome will constitute only a part of a chromosome at prophase, metaphase, or anaphase. This opinion, which is **Rosenberg's** (1904), was supplemented by **Grégoire** (1907), for whom the prochromosome or euchromocenter corresponds to the median part of the chromosomes; indeed, according to **Heitz** (1929), prochromosomes correspond to the arcuate part of anaphase chromosomes.

The reader will see in plate 41, at numbers 16, 18, 25, of fig. 3, for example **(1)**, a series of chromocenters applied against the nuclear membrane in the interphase, then in plate 42, fig. 1, at numbers 177, 178, 185, 186, 187, 188 **(2)**, the dumbbell shape that this small round or oval granulation called euchromocenter would take in metaphase or anaphase.

As for the constitution of the nucleolus, it is completely unknown, and we are not yet well established either on its role or on its evolution during kinesis. For some authors, it would be divided into two fragments which would form the nucleoli of the two new nuclei. For others, the nucleolus would disappear completely during kinesis, but would reappear at each pole in telophase.

(1) *Figures borrowed from the thesis of P. Dangeard (3 bis).*
(2) *Figures borrowed from the thesis of Jenny Doutreligne (3 quarto).*

According to **Jenny Doutreligne** (13, p. 34):

Often, mainly in objects with reticulated nuclei, the nucleolus disappears entirely before the formation of the metaphase figure. Elsewhere, the nucleolus is seen to diminish during prophase, but without disappearing entirely; a more or less voluminous portion remains, which is located in the equatorial plate, or in the vicinity of it, and which, after having reached one pole or after having been divided, without regularity, between the two poles, fragments into parcels which spread into the protoplasm and disappear there.

According to most authors, the nucleoli would only appear at the end of telophase. According to some authors, the decrease in volume of the nucleolus during prophase would result from its participation in the formation of chromosomes, but this is only a hypothesis. We will see later the great importance of these observations and how clearly they receive a rational explanation. Let us now turn to our personal observations.

* * *

Influence of Fixing Reagents on the Results of Microscopic Observations

The constituents of the plant cell nucleus are the same as those of the nucleus of animal tissue cells.

As with the study of the animal cell nucleus, I did not make any special preparations for the study of the plant cell nucleus. Having noted that the cytoplasm of the plant cell is more sensitive than the nucleus to the altering action of fixing liquids, I sought the one of these liquids that appears to cause the least alterations in the cytoplasm.

A study of mitochondrial fixatives will be found on the page, and the reasons why I chose 10% (one-quarter) formalin **Ringer's** or **Locke's** liquid as a fixative will be seen.

With this fixative, the plant tissues, although altered, were sufficiently well preserved, as demonstrated by the photographs in plates 1 to 11, to allow the constitution of the cytoplasm to be established with certainty.

Although formalin-coated **Ringer's** solution gave me better results than other fixatives, it nevertheless causes alterations in the nucleus as well as the cytoplasm.

This point is of great importance, because we will see that it is the alterations due to the action of fixatives that have prevented us from seeing the exact structure of the nucleus until now, and which have been the cause of erroneous conclusions.

P. Martens (45) sought to visually assess the action of fixatives. From his observations on the fixation of nuclei at rest or in interphase carried out under the objective with uninterrupted control of the observer, he concluded that the fixative specifies and accentuates without modifying the filamentous reticular structure already observed on the spot, that it does not create any new structure and does not cause any displacement or upheaval of the formed elements; this conclusion further implies that the elements thus observed are not altered or destroyed by the fixative. **Martens'** observations are certainly correct, but the following objections can be made to his conclusions: firstly, he was not able to specify at what moment the cells he subjected to the action of the fixative were actually fixed and he gives no details on this subject; secondly, such a short-term action of the fixative (because, although **Martens** did not provide details on this subject, these are only short observations, probably lasting several hours), is not comparable to that of a slow fixation of 24 to 48 hours, for example. In the latter, the altering action of the fixative has plenty of time to manifest itself, whereas **P. Martens** was not able to observe it in his observations, which were too short for it to have time to appear clearly.

From **Martens'** interesting observations, it must be remembered that it is certain that fixative liquids do not create new figurative elements or new structures from scratch; but it must not be forgotten that they can, by their precipitating or coagulating action, modify the appearance or apparent form of figurative elements or of a normal structure, and even that such modifications are frequent.

Figured elements can be completely coated by coagulated or precipitated matter and take on a shape very different from their normal form. Thus, in the same field, we can see chromosomes altered by the fixative, becoming threadlike, while others, coated in a coagulum, become massive or shapeless. This fact is easy and common to observe if we carefully examine the drawings of the many authors who have studied chromosomes during kinesis. We will see examples of this later.

We cannot doubt the destructive action of fixatives after the multiple demonstrations that have been given here regarding the structure of the cytoplasm, or regarding the nature of the plastids of *Elodea Canadensis.*

The fixatives most commonly used in plant cytology, that is, **Flemming**-type fixatives, strong or weak, or even with very little or no acetic acid (**Benda, Mèves**, etc.), **Helly**, **Regaud, Champy** mixtures, etc., have a sufficiently profound altering action to completely disorganize the structure of the cytoplasm and the nucleus and profoundly alter or even destroy their formed elements. Plant tissues are much more sensitive to this alte-

ring action than animal tissues. And, among plant species, some have tissues that are considerably more alterable than others.

Almost all observers who, over the past 25 to 30 years, have studied the constitution of the nucleus of plant cells and karyokinesis have adopted the rule of using the same fixing reagents as their predecessors, and this because, according to the same constantly repeated assertions, these liquids are those that best respect the elements of the nucleus of the plant cell. But all these liquids strongly alter the cytoplasm and the elements of the nucleus. It is as a result of this alteration that observers have described as homogeneous nuclei that were subsequently seen to be organized, for example, that of Elodea Canadensis, which is the first in which I was able to observe the radiating structure of the nucleus and that of the nucleolus.

The same is true for *Lathrea clandestina*, classified among the species with homogeneous nuclei, that is, containing only chromocenters and which nevertheless contains the tracts connecting these chromocenters to the nucleolus that **P. Dangeard** (11 A) represented in figure 27, plate 31 (fig. 3, no. 27 of pl. 42 of this 3rd volume) of the memoir analyzed further on. In reality, there are no more structureless nuclei than there is cytoplasm without a network of dumbbells, and when this structure does not exist, it is, without exception, because it has been destroyed by the preparation processes. The constant alterations of the cytoplasm by fixatives prove that it is impossible for the elements constituting the nucleus not to be themselves altered by them.

In a fairly recent note on the counting of chromocenters in the interphase nucleus, **P. Dangeard** (11 B) noted *that Regaud often does not allow chromocenters to be detected in nuclei that nevertheless contain them.*

This simply means that **Regaud** destroys chromocenters just as it destroys cytoplasmic dumbbells.

Structure of the Nucleus

Before reporting these observations, I must recall the general principles that emerge from the studies conducted so far and which must find their application in the study of the nucleus in plants.

By studying the constituent elements of the cytoplasm of the plant cell, I have shown that the elements that have been considered until now as chondriosomes, and to which have been attributed either a spherical shape (granular chondriosomes), or a rod-like shape (chondrioconts), or a club-like shape, do not have an autonomous existence and are only the debris of the destroyed cytoplasmic organization. In an intact cell, the cytoplasmic organization is built by a dumbbell-shaped organelle consisting of a rod (chon-

driocont) bearing at each of its ends a granular chondriosome. Moreover, these dumbbells are always articulated with each other by their balls to form the cytoplasmic network.

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In an unaltered, intact cell, there are no isolated dumbbells, and there is never a single rod without its balls. In an intact cell, a dumbbell is always articulated by its balls with others, or with the external cell membrane, or with the nuclear membrane.

We have seen, moreover, that in the youngest cells of the meristems, the cytoplasmic organization, which is very simple and has remained unnoticed until now, comprises a single row of dumbbells, all arranged exactly along the extension of the radius of the nucleolus, one of whose balls is attached to the nuclear membrane.

As the cell ages, the structure of the cytoplasm becomes more complicated. A second row is formed, then several other rows of dumbbells, which, articulating with each other by their balls, form the cytoplasmic network, the constitution of which is identical to that of the linin network of the nucleus.

Two types of nuclei are generally accepted: nuclei with a reticulated structure possessing the characteristic linin network and nuclei of the euchromocentric type or with prochromosomes, possessing only the nucleolus and granules arranged at the periphery against the nuclear membrane; these are called homogeneous nuclei because they are optically empty, that is, without figured elements in the space located between the euchromocenters and the nucleolus.

Structure of the euchromocentric type

The euchromocentric type is seen in the interphase nuclei of meristems, that is, in the very young cells of many species. It will be demonstrated that the structure of these euchromocentric nuclei is, in very young interphase cells, identical to that of the cytoplasm surrounding them and comprises a single row of radiating dumbbells, one of the balls of which is articulated at the periphery of the nucleolus and the other at the nuclear membrane; the latter is **Gregory's** euchromocenter.

This identity between the structure of the cytoplasm and that of the nucleus in the young meristem cell leads one to think of an identity in the subsequent development of these two parts; the cytoplasm of the young cell later taking on the reticulated type, the nucleus with a single row of dumbbells of this same cell must also, as it develops, later take on the reticulated type. Let us now move on to the demonstrations:

Plate 38 shows photographs of a series of nuclei in the young leaves of the axillary bud or stem tip of *Elodea Canadensis* and, for Figures 8 and 13, in the radicle (about 12 mm long) of the germinating bean. By careful examination of these figures under a magnifying glass, we see:

1. That the nucleus of the plant cell at rest is made up of dumbbells having exactly the position of the radius of the sphere-nucleus, one of the balls which corresponds to the euchromocenters being pressed against the nuclear membrane and the other, unknown until now, being included in the nucleolus. These dumb- bells form a regular radiating crown all around the nucleolus;

2. That the peripheral balls of the dumbbells, pressed against the nuclear membrane, are spherical, very regular and not flattened or oval as **P. Dangeard** depicted them, for example, in figures 15 to 34 of plate XXXI of his memoir (11 A); if the balls had the shape he drew, it is because they were impastoed by a chromatic material which remai ned adherent to them and which fills the angular space which exists between each ball and the nuclear membrane. **P. Dangeard** has also depicted other regularly spherical balls, especially those which are not attached to the nuclear membrane, and we do not see any reason why certain balls should be ovoid and others spherical;

3. That the rods of the radiating dumbbells are destroyed in certain sectors of the nuclei, even entirely in several, this as a result of the altering action of the fixative.

Figures 10 and 11, plate 38, are photographs of the same nucleus with two different focuses; they show with all desirable clarity one of these dumbbells whose external ball is located against the nuclear membrane (euchromocenter) and whose internal ball is hidden in the nucleolus.

On the other hand, Figure 8, plate 38, carefully examined under a magnifying glass, shows on the left side of the nucleus the radiating crown of dumbbells and their external balls pressed against the nuclear membrane.

The figures on this board therefore demonstrate:

1. That the elements which enter into the constitution of the nucleus are dumbbell organelles absolutely similar to those which constitute the cytoplasm;

2. That, in nuclei with a simple structure of the euchromocentric type, these dumbbell organites have a radiating arrangement around the nucleolus;

3. That this structure has the effect of maintaining the nucleolus in a fixed position at the center of the cell and ensuring the conservation of the spherical shape of the nucleus.

Structure of the nucleolus of euchromocentric nuclei

The research presented in the second volume of this work and all the knowledge acquired so far in this third volume on the subject of the cytoplasm have taught us that the construction of the organism of all living beings is carried out by a single, universal organelle, the dumbbell, a rod equipped with a granulation at each of its ends; they have shown us that dumbbells never exist alone, isolated. A dumbbell is always articulated with others by its balls; they have also shown us that the dumbbell is always complete and that the club-like shapes, which only exist in cells altered by fixatives, are fragments of dumbbells released by the dislocation of the cytoplasmic network.

In the little altered regions of the cytoplasm and the nucleus, the dumbbells not destroyed by the fixative have a ball at each end of the median rod; often, one of these balls is hidden in a mass, either of chromatic substance, or of coagulated or precipitated matter, or of lipoid substance. Also, when this fact occurs, the rod of the dumbbell and the ball it carries having remained intact, one can be sure that the second ball, not visible, has also remained intact. When it comes to the nucleus, this fact is the rule, the central ball of the radiating dumbbells being included in the nucleolus and generally not visible.

It does not appear easy, at first glance, to demonstrate this fact, that is, to provide direct proof of the existence of the internal balls of the radiating dumbbells. The tips of the stems or buds of *Elodea Canadensis* and the radicle of a germinating bean, fixed with formalin **Ringer's** solution and stained with ferric hematoxylin, show nuclei in which the rods of the radiating dumbbells are distinctly visible around the nucleolus; this is what Plate 38 shows in almost all its figures. The balls of these dumbbells cannot be distinguished if the staining of the nucleolus is too intense; but if the hematoxylin has been applied sparingly, it can be seen that the central balls of the radiating dumbbells form the peripheral part of the nucleolus and, consequently, the greater part of its mass.

These central balls of the dumbbells are seen in Figures 4, 5, 6, and 7, Plate 38; in the upper part of the central nucleolus in Fig. 4, we see 6 to 7 balls, two of which belong to two complete dumbbells with very short rods. In Figure 5, we see the central balls of the dumbbells over almost the entire periphery of the nucleolus, and at least three or four of these dumbbells show a very short rod.

In other cases, the central balls of the radiating dumbbells are only detected by a bump on the surface of the nucleolus, which is reflected at its periphery by a more or less pronounced festoon. This festoon is noticeable around most of the nucleoli in Plate 38; three of the nucleoli in Figure 1 are very interesting in this respect.

In this plate, we can also clearly see the rods of the dumbbells and their radiating arrangement which, very visible in almost the entire periphery of the nucleolus in figs. 1, 2, 3, 5, 8, 9, 11, 12 (*Elodea*), plate 38, is only seen in other figures in one sector of the nucleus, the fixative having destroyed part of the dumbbells.

P. Martens, in a memoir on the study of kinesis in the living cell (16, 4) made the following remark about the clear areola surrounding the nucleolus: *This areola facilitates observation of the nucleolar surface, which is rather finely polyhedral and covered with facets, than perfectly spherical.*

These facets, which are only apparent and not real, probably correspond to the appearance given to the surface of the nucleolus by the central balls of the radiating dumbbells which are included there and embedded in a layer of homogeneous and semi-transparent chromatin. They correspond, but in another aspect, to the festoons which are noticed around the nucleolus, when the external edges of the dumbbell balls which constitute its periphery are a little visible, as in plate 39 (figs. 1 and 2) for example. In summary, the central balls of the radiating dumbbells form a regular layer at the periphery of the nucleolus.

In the photograph used to print figure 5, plate 38, we see other details of great interest, which are rare to be able to distinguish; they are not visible in this figure 5 because it is a photograph printed with a strong exposure to clearly bring out other elements. To be able to distinguish the details of the photograph, it is necessary to print various lighter prints, with a varied exposure time, like those of plate 39; we can also examine the photo graph directly. Under these conditions, we can first distinguish the first row of balls at the periphery of the nucleolus, that is to say the central balls of the radiating dumbbells; then, placed against these and more strongly colored, a second row of balls of the same size, which we can distinguish especially at the upper part of the nucleolus in figures 5 and 6, plate 39; Finally, in the very central part of the nucleolus, we can clearly distinguish, in figure 6, other balls of the same size as those of the periphery; these balls, like all the other elements shown of the nucleolus, are surrounded by a strongly colored, homogeneous material, which bathes them.

The proof of the reality of this constitution of the nucleolus is provided by the direct examination of the living cell and without the aid of any coloring. This is one of the rare cases where vital observation has provided me with valuable information.

Indeed, Figure 5 of Plate 12, which is a photograph of a young leaf of *Elodea Canadensis* (750 g/m^2) placed between a blade and a coverslip and in the water where it was developing, shows, at the top left, a nucleus with a large nucleolus at the periphery of which

a ring of granulations of a fairly dark color can be seen; inside this ring, a second ring of very refracting granulations can be seen, appearing as bright points, and finally, in the central part, other scattered refracting granulations.

The constitution of the external layer of the nucleolus by the balls of dumbbells appears with perfect clarity in the nuclei of the cells of the liver of the rat. A fragment of this organ being fixed for 3 or 4 days in 10% formalin (one-quarter formalin) or in formalinated Locke's fluid, we notice numerous nuclei possessing a reticulated structure and several nucleoli around each of which we clearly distinguish the rods of a certain number of radiating dumbbells but whose balls adherent to the nucleolus are not perceptible, or only by a slight circular festoon when we stain the preparations with hematoxylin; on the contrary, when we stain them by the **Feulgen** method, the perinucleolar balls of the dumbbells stand out clearly in bright red at the periphery of the nucleoli and, from each of them, a weakly colored rod leaves whose direction is always strictly that of the radius of the nucleolus; This proves that the substance that masked these balls in the previous case is not chromatin. In the memoir by **J. Doutreligne** already cited (13), the following passage, relating to the nuclei of *Calicanthus floridus*, shows that it is the same in plants:

The nucleolus is voluminous, colored black by hematoxylin. We can also distinguish the two regions that we described in Luffa: a lighter central region and a darker peripheral region. Often, in the ***Feulgen*** *material, the nucleolus seems to contain small enclaves at the boundary of the two regions that take on the same red tint as the euchromocenters. But this detail becomes especially visible in the prophase nuclei.*

These small peripheral enclaves, which take on the same red hue as the euchromocents, are obviously the central balls of the radiating dumbbells, of which the euchromocenters are the peripheral balls. It also seems obvious that these balls should be seen at the periphery of the nucleolus mainly in interphase and not in prophase, because it would be surprising if they did not detach from it during prophase to participate in the creation of a chromosome like the other ball of the dumbbell to which they belong (euchromocenter).

The fact of the decrease in the volume of the nucleolus during prophase, which has now become certain, is perfectly explained by the detachment, from the periphery of the latter, of the central balls of the radiating dumbbells, these balls accompanying neces-

sarily the peripheral balls of the latter (euchromocenters) in the confection of the chromosome. We will see further on that the chromosome contains complete dumbbells, one, two or three of which, sometimes visible at their extremity, have been called satellites (**Navachin**, 1912).

As for the balls of the central region of the nucleolus, it is impossible to discern in Figs. 5 and 6, Plate 39, whether they are balls of internal dumbbells or balls of the periphery of the nucleolus seen from the front or even simple granulations.

* * *

In summary, the facts just presented establish:

1. That most observations relating to the structure of the nucleus at rest or in interphase are tainted with error because the fixatives used have altered or even destroyed the figured elements it contains;

2. That the presence of a nucleolus in a nucleus that does not contain other figured elements allows us to affirm that they existed normally and that, if they have disappeared, it is because they have been destroyed by the fixative;

3. That in a nucleus provided with a nucleolus and euchromocenters (peripheral granules) not connected to it by rods, the latter have been destroyed; their absence constitutes proof of a profound alteration of the nucleus by the fixative;

4. That during fixation, the precipitation or flocculation of substances occurs which are deposited on the figured elements and can modify their form or conceal their existence.

We will be able to verify the reality of these alterations by analyzing an interesting memoir by **P. Dangeard** (11 A) and show at the same time that, even in nuclei in which only a few figurative elements are partially preserved, we find proof of the preexistence of the normal structure that has just been described, but which escaped this observer. I add that, if we can see that this normal structure has thus remained partly apparent, it is thanks to the author's concern for accuracy, very visible down to the smallest details, and to the precision of his drawings and descriptions.

In this memoir, the structure of the nucleus of various plants is studied during interphase and during kinesis. We will only deal here, exclusively, with the structure of the interphase nucleus.

Let us say at once that the author's drawings of the resting nuclei of nine of the plants he studied demonstrate that the structure of the nucleus is completely or largely destroyed, except in the rare cases where vestiges remain, which are nevertheless characteristic enough to allow us to state with certainty that the normal structure existed there. Here are some details:

The nuclear structure is completely or almost completely destroyed in the following plants (the numbers of the figures and plates indicated in each case are those of **P. Dangeard's** memoir):

1. ***Radicle of Vicia Faba*** (figs. 1, 2, 3, 7, pl. XXVI). There is total destruction of the nuclear organization in these four nuclei. This organization exists, however, and can be detected by fixing the radicle with a 10% formalin solution or with **Ringer's** fluid containing 10 or 12% formalin. It is represented in plate 75 of this volume.

2. ***Tradescantia***, figure 6, plate 26 (**Benda, Mèves**).

3. ***Aillum Cepa***, radicle, figures 8 to 12, plate 26 (**Benda, Mèves, Regaud**).

4. ***Pisum sativum***, radicle, plate 26. In figures 13 to 17, the structure is completely destroyed by the fixative (**Bouin**). In figures 27, 28 (figures 10 and 11 of plate 42 of this volume), 29 (**Benda, Mèves**), the destruction is not complete; a more or less long portion of the dumbbell rods remains, adhering to the nucleolus and proving, indisputably, that the radiating organization existed in these nuclei.

5. ***Oxalis stricta***, figure 13, plate 27 (**Regaud, Feulgen**).

6. ***Elodea Canadensis*****,** plate 27, figures 15, 16 (**Bouin**), 20, 21 (**Benda, Mèves**) interphase nuclei, 17, 19, quiescent nuclei (**Bouin**).

7. ***Ricinus communis***, plate 29, interphase nuclei. In figures 2, 19, the organization is totally destroyed; small euchromocenters remain in figures 1, 3, 17, 18. There are three short fragments of dumbbell rods adhering to the nucleolus in figure 25, and a complete dumbbell in figure 26, the second ball being included in the nucleolus (**Bouin, Helly, Benda, Mèves**). The destroyed nuclear structure will still be found, with the exception of tiny euchromocenters, i.e., in the process of disappearing, in:

8. ***Cucumis melo*** (pl. 31) *Cucurbita pepo* (pl. 31). In Figure 5 we see a *Cucumis melo* nucleus no longer containing any peripheral chromocenters, although there were some, since the nucleolus shows at its periphery the stumps of three dumbbell rods, part of which has consequently been destroyed by the fixative, as well as the peripheral ball.

According to **P. Dangeard,** Fig. 16 of Pl. 31 (*Cucumis melo*) represents the beginning of chromosome formation. This figure very clearly represents the dislocation of the nuclear network; but it is difficult to say whether this is a dislocation produced by the fixative or the natural dislocation that marks the beginning of the formation of chromosomes at the beginning of prophase.

9. ***Arum italicum***, Pl. 34 (**Helly**). In other observations by **P. Dangeard**, the nuclear structure has not been completely destroyed, and in some, such as in the nuclei of *Lathrea clandestina*, sufficient elements remain (nuclei nos. 15 to 34, plate 31) to perfectly and very clearly recognize the nuclear structure I described above.

This structure is characterized by the dumbbell organelle occupying exactly the position of a radius of the nucleus and whose peripheral ball is pressed against the nuclear membrane, while the central ball is included in the nucleolus. A certain number of these dumbbells are seen around the nucleolus, in a radiating arrangement.

The various drawings of *Lathrea clandestina* nuclei in this plate 31, which I have reproduced in figure 3, plate 42 of this volume, show them in various states of alteration, the least altered being that of figure 27, in which five dumbbell rods can still be seen; but, in figure 26, only one can be distinguished. These drawings, by their accu- racy, provide proof that, when a small fragment of rod remains adherent to the nucleolus, it indeed represents the remainder of a disappeared dumbbell rod. Indeed, in figures 15, 23 and 24, we see dumbbells of which a portion of the rod has disappeared, altered by the fixative, and of which the remaining parts have remained opposite each other, the part remaining attached to the nucleolus being exactly opposite the peri- pheral ball which is a euchromocenter. **P. Dangeard** himself has also indicated that this solution of continuity can be attributed to the action of the fixative.

In the other plates of **P. Dangeard's** memoir, we find numerous nuclei with a very altered structure, but still preserving traces of their organization, even if only one or two short pieces of dumbbell rods attached to the nucleolus or the euchromocenters, and which are sufficient to prove the existence of the radiating organization and its destruction by the fixative. Such vestiges are visible in the following figures:

Rhinantus major, figures 16, 18, 19, plate 33.
Terminal bud of Bryonia dioica, figures 34, 35, plate 33.
Radicle of *Cucurbita maxima*, figures 10, 12, 13, plate 31.
Terminal bud of *Lathrea clandestina*, figures 15 to 34, plate 31.
Radicle of *Tropeolum majus*, figures 1 to 3, 34, 37, plate 30.
Radicle of *Lupinus albus*, figures 16 to 18, plate 30.
Radicle of *Ricinus communis*, figures 25, 26, plate 29.
Bud of *Elodea canadensis*, figure 24, plate 27.
Radicle of *Pisum sativum*, figures 27, 28, plate 26.

On page 296 of the memoir cited above, **P. Dangeard** considers these vestiges of nuclear organization as follows:

Since 1934 we have published various preliminary notes on chromocentric nuclei. The origin of this research lies in the cytological study of Lathrea clandestina that we carried out in Bordeaux (1934) (6); in this plant, in fact, the nuclei are very particular due to the presence of bulky chromocenters located for the most part against the nuclear membrane.

*Some of these chromocenters are connected by a pedicle to the nucleolus, and the fact is so striking that one has the impression of budding of these elements from the nucleolus. There are, for example, chromocenters which are attached to the nucleolus by a very fine tract which gradually becomes thinner; other chromocenters appear to have recently detached from the nucleolus. The use of **Feulgen's** method, however, showed us shortly afterwards that the nucleolus and the chromocenters do not react in the same way and that, consequently, the relationships between nucleolus and chromocenters cannot be interpreted in the sense of nucleolar budding (1934) (7).*

And he concludes thus:

This is not specific to Lathrea, and in many nuclei with chromocenters, we have observed the connection of one or more chromocenters with the nucleolus. It is difficult to know what these relationships between nucleoli and nuclear chromatin mean. They undoubtedly respond to phenomena of adhesion between the nucleolar substance and the chromatin, rather than to an exchange of substance occurring between these two formations. However, we cannot affirm anything.

P. Dangeard, who was unaware of the existence of the universal organelle dumbbell and its fundamental role in the construction of beings, could not, for this reason, see or sense that this pedicle, which connects the nucleolus to the chromocenter, is the rod of a dumbbell whose chromocenter is the peripheral ball and whose central ball, included in the nucleolus, is difficult to see because of the strong coloration that it takes on by the coloring reagents.

As for the conclusion relating to the different reaction of the chromocenters and the nucleolus to the **Feulgen** method, it calls for the following comments:

1. In the first place, the chromocenter, the rod of the dumbbell and its central ball, included in the nucleolus of which it is a part, are the constituents of a single organelle, and it seems certain that the two balls of this organelle react in an identical way to the same reagent;

2. I recall that **J. Doutreligne** (14) reported that, in the peripheral intranucleolar region of *Calicanthus floridus*, we often notice, after **Feulgen** *staining, small enclaves that take on the same red tint as the euchromocenters*, and that these small enclaves are obviously the central balls of the radiating dumbbells included in the peripheral zone of the nucleolus.

3. The dumbbell organelles, whether those of the nucleus or those of the cytoplasm, often have one ball quite strongly stained black by hematoxylin and the other little or

not stained, sometimes even so deprived of staining that it appears as a bright, very refractile ball with indistinct contours.

We have seen previously that there are two substances in the dumbbell: the fundamental, fixed, hyaline, achromatic substance, which forms the skeleton of the dumbbell and which is what is designated by the word neurokeratin in the network of the myelin sheath of nerve fibers, then a chromatic substance which impregnates this fundamental substance in very variable quantity, and appears to be able to move inside the organelle, and even leave it to pass into another or into a bud in formation. In the nuclear dumbbell, this chromatic substance is formed by chromatin which, alone, is colored red by the **Feulgen** nucleal reaction.

In addition, there exists in the nucleus a second chromatic substance that does not stain red by the **Feulgen** nucleal reaction, and which is therefore different from chromatin, but which stains like it black, by ferric hematoxylin. This second substance permeates the nucleolus, also exists in the nuclear enchylem, but in much smaller proportion, and also in the nuclear dumbbells.

It is this substance which, alone or mixed with an achromatic substance, is deposited on the chromosomes during anaphase and gives them the shape of clubs, as demonstrated by **Jenny Doutreligne**. By **Schæde's** fuchsin-iodine green, it stains red, while true chromatin stains blue.

It is likely this same chromatic substance that impregnates the cytoplasmic network and allows it to be stained black by hematoxylin. The cytoplasmic network does not stain bright red by the **Feulgen** reaction, but it nevertheless stains pale pink, sometimes very distinct pink, which suggests that it may be the achromatic substance that causes this reaction, since the chromatic substance of the network does not.

* * *

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In addition to this connection of the nucleolus with the peripheral euchromocenters **P. Dangeard** indicates in the following passage of this same memoir (3 bis) other corpuscles attached to the nucleolus:

With a little attention, we can distinguish in Lathrea other corpuscles which are probably not formed of chromatin; their shape is globular. They correspond to the definition of micronucleoli. In Arum italicum, the small, regularly rounded protuberances observed on the nucleoli also correspond to micronucleoli generated by the main nucleolar body and which can subsequently detach. Elements of the same nature are still found in many other plants….

P. Dangeard then formulates the hypothesis that these micronucleoli could represent centrosomes that have more or less lost their directive function in mitosis.

The non-peripheral corpuscles connected to the nucleolus by a short rod (no. 22, fig. 3, pl. 41) must be the external balls of short dumbbells whose central ball is included in the nucleolus and whose external ball was probably articulated with the central balls of one or more other dumbbells whose other, peripheral, ball was attached to the nuclear membrane. Such a device can be seen in figure 25. In figure 22, two short dumbbells exist exactly opposite a peripheral chromocenter. In nucleus no. 28 of plate 31 of **P. Dangcard's** memoir, not reproduced in our plate 41, we see two groups of two dumbbells articulated end to end, one of the balls.

One end is articulated in the nucleolus, the other against the nuclear membrane. As for the perinucleolar corpuscles (3 bis, fig. 4, p. 362) of *Arum italicum*, plate 39 of this book provides exact information on their nature and role; they are the central, perinucleolar balls of the radiating halteres.

The ancillary bodies, isolated granulations attached to the periphery of the nucleolus that, in a later memoir (3 ter), **P. Dangeard** described in the kernels of the bean and the radish, are of the same nature.

* * *

We will conclude this presentation by summarizing the facts and observations that have just been presented with the following conclusions:

1. The nucleus of plant cells always has an organization consisting of an elementary organelle of a shape identical to that which constitutes the cytoplasm: the dumbbell shape;

2. The nuclear dumbbell organelles always assemble themselves together by their balls, like those of the cytoplasm and form by their assembly, the nuclear network with polygonal meshes of which each side is formed by a dumbbell and each corner vertex by several dumbbell balls (3 to 5 or 6) placed against each other (chromocenters); this constitution is rigorously identical to that of the cytoplasm;

3. Just as the cytoplasmic network is articulated on the one hand with the cell membrane, on the other hand with the periphery of the nucleus, the nuclear network is articulated on the one hand with the nuclear membrane, on the other hand with the periphery of the nucleolus; this articulation is carried out exclusively by the balls of the dumbbells;

4. The dumbbells of the network which are articulated with the nucleolus have their rods placed exactly in the extension of the radius of the latter and their nucleolar balls included in its mass, of which they constitute the peripheral zone;

5. The dumbbells of the network which are articulated with the nuclear membrane have only one of their balls pressed against this membrane and their rod in the neighboring direction of that of the nuclear radius;

6. The simplest type of nuclear organization (euchromocentric or non-reticulated type with prochromosomes) is made up of a single row of radiating dumbbells that have exactly the direction of the nuclear radius, their peripheral balls pressed against the nuclear membrane (euchromocenters or prochromosomes) and their central balls articulated in the nucleolus of which they constitute the peripheral zone.

This type of nuclear organization corresponds exactly to the type of cytoplasmic organization with a single row of dumbbells that exists in very young meristem cells, *a type that is always evolving and transforms* in older cells into a network of 2, 3, then 4..., etc., rows of dumbbells and polygonal meshes.

This fact indicates, by analogy, that the type of nuclear organization with a single row of radiating dumbbells (euchromocentric type) must probably also evolve and transform into the reticulated type in cells at definitive rest;

7. One of the roles of the nuclear network and the radiating dumbbells is to maintain the shape of the nucleus and to fix the nucleolus in its central position. The presence of a nucleolus therefore necessarily implies the existence of a network or row of radiating dumbbells, that is, of a nuclear organization, *from which it follows that it is impossible for there to be homogeneous nuclei* comprising only independent granular elements unconnected to the nucleolus and the nuclear membrane. Any nucleus possessing a nucleolus necessarily possesses a nuclear network (the simplest form of which is the single row of radiating dumbbells) consisting of dumbbells whose balls are the chromocenters and prochromosomes (euchromocenters).

8. The peripheral balls of the radiating dumbbells (euchromocenters or prochromosomes), and the groups of 2 to 5 or 6 dumbbell balls located at the vertices of the angles of the polygonal meshes of the nuclear network (chromocenters) are of exactly the same nature, the same origin, and the same constitution. Only their size varies according to the number of dumbbell balls that constitute them, and according to the size of these dumbbells.

The prochromosomes are therefore not, as **A. Eichorn** thinks, different formations perfectly distinct from the chromocenters. They are identical formations, but incomplete because they are only a part of the dumbbells to which they belong.

9. All fixatives significantly alter the nuclear structure. They swell, then dechromatinize, and finally dissolve the balls and rods of the dumbbells. This is why the direct connection between the euchromocenters and the nucleolus has remained unknown until now. It is also why the number of euchromocenters or prochromosomes, partly destroyed by the fixative, is often much lower than the number of chromosomes.

The ***homogeneous*** state observed in certain nuclei is due to the alteration caused by the fixative, for example in *Cyclauthera pedata* and *Oxalis stricta*. These nuclei, possessing a nucleolus, certainly possess, as a result, a structure consisting of dumbbells intended to hold it in place. A fixative less harmful than the fixatives used until now will certainly display this structure.

EVOLUTION AND SHAPES OF CHROMOSOMES DURING KINESIS

During this presentation, we will successively study:

The formation of chromosomes in the euchromocentric type.

The formation of chromosomes in the reticulate type.

The nature of satellites and their supposed cleavage.

The nature and role of chromomeres.

The counting of chromocenters and chromosomes.

The accessory bodies and micronucleoli.

The classification of nuclear types.

To clarify this presentation, it is necessary to recall the form of the nuclear structure at the beginning of kinesis, that is, during the quiescent or interphase state. At this time, the nuclear structure can be achieved in two different forms, by the assembly of dumbbell organelles connecting the nucleolus to the nuclear membrane:

1. By a network of dumbbells (reticular type) articulated by their balls, either among themselves to form the polygonal meshes of the network, or to the nuclear membrane and the periphery of the nucleolus to fix the network there;

2. Or solely by a single row of dumbbells (euchromocentric or radiating type) placed exactly along the radius of the nucleus and articulated, on the one hand, to the nuclear membrane by their peripheral balls which correspond to the euchromocenters and, on

the other hand, to the nucleolus by their central balls which constitute the peripheral part of it.

The organization of these two types by dumbbell organelles has remained unknown until now, as has the radiating structure of the euchromocentric type and that of the nucleolus.

It is therefore these dumbbells, of which the chromocenters and prochromosomes are only the balls or points of articulation, which are the formative elements of the chromosomes; let us note further that, in these two types of nuclei, the structural disorganization which occurs at the beginning of prophase leads to the mobilization of the dumbbells and their balls and, consequently, *the dislocation of the peripheral zone of the nucleolus, which results in a significant reduction in the volume of the latter.*

The constitution of chromosomes in the euchromocentric or radiant nuclear type is still very little known; it is mainly this type that appears to give rise to short chromosomes. These often appear to retain the dumbbell shape, at least in certain phases of kinesis. Several examples can be found in Figures 8, 9, 12, 13, and 14 of Plate 41, taken from a memoir by **Björn Foyn** (21) and representing heterotypic prophase in the sexual cells of a green alga: *Cladophora suhriana* (*Phaeophyceae*). In particular, Figs. 9 and 13 show two chromosomes formed by a thin, arched filament bearing a ball at each end; in Fig. 8, we see gemini whose two filaments are terminated by dumbbell balls.

In figure 1, plate 42, reproduced from the memoir of **J. Doutreligne** (13) we see, in numbers 178 (*Impatiens Balsamine*) 185, 186, 187, 188 (*Scandix pecten-veneris*, anaphase) chromosomes having a clear dumbbell shape; this shape may be due to the fact that the chromosome which has an arched and slightly clubbed shape is seen from the pole. But this club may be due to the fact that a dumbbell ball exists at each end of the chromosome and that the achromatic substance which is deposited on the latter consequently makes its ends wider. Let us note, moreover, that in figure 136 of the treatise on plant cytology by **Guilliermond, Mangenot, Plantefol**, reproduced in fig. 2 of this book, we see 5 *Muscari* chromosomes ending in satellite dumbbells (***b*** to ***f***) and another very short one g formed only of two dumbbells placed side by side.

We will see later in the study of the nature and origin of the satellites that these are the extremity, now visible, of the dumbbell filaments constituting the substratum of the chromosome, which implies that, in chromosomes without satellites, these dumbbells also exist but are made invisible by the achromatic and chromatic materials deposited on them. This indicates to us at first sight that, despite the difference that exists between the structure of reticulate nuclei and that of nuclei of the euchromocentric type,

the structure of the chromosomes of these two nuclear types must probably be the same and include a substratum constituted by dumbbells.

The great difference in the constitution of these two types of nuclei results in two modes of formation and evolution of chromosomes that must be studied separately.

Formation of chromosomes in nuclei of the euchromocentric or radiating type.

For **Kühn** (1929), the chromocenters disappear in the nuclear medium, and new formations appear in this medium, which is completely devoid of figurative elements.

For others (**Schiller** 1928, **Eichorn** 1930), the chromocenters or prochromosomes do not undergo any transformation during kinesis and are found in the same state afterward.

For **Grégoire** (1907), **Heitz** (1929), **J. Doutreligne** (1932), and others, the euchromocenters participate in the formation of chromosomes, but constitute only the narrowed median part, which, according to them, corresponds to the constriction of insertion on the spindle fibers.

It is doubtful that this opinion can be correct because, the euchromocenter being only the external ball of a radiating nuclear dumbbell, it does not take into account of the role of the latter's rod or of its central intranucleolar ball in the formation of the chromosome.

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The existence and nature of this rod which connects the euchromocenter to the nucleolus has escaped all observers. Only **P. Dangeard** saw and depicted this rod, particularly in the nucleus of *Lathrea clandestina*, but considered it an inconstant formation whose function and nature he could understand neither, because he was unaware of the existence of the dumbbell organelle and its role in the organization of living beings.

The thesis of **Kühn** and certain authors who admit that the euchromocenters dissolve, either at telophase or at prophase, in the nuclear background, since the chromosomes begin to appear immediately afterward in the form of filaments, is contrary to currently known facts. The vast majority of observers have noted the presence of euchromocenters in interphase and at the beginning of prophase and have been able to follow their continuous evolution.

The disappearance of the formed elements in interphase or at the beginning of prophase can currently only be considered as due to the altering action of the fixatives, which is of capital importance here; it always occurs regardless of the fixative used; its action is irregular on similar elements of the same nucleus: some may be completely destroyed,

others only lose, totally or partially, their ability to fix the dyes; the rod of the dumbbells is the part most sensitive to alteration and often disappears completely.

Another observation difficulty results from the differentiation carried out after staining with hematoxylin; the parts altered by the fixative and which have lost most of their ability to stain remain invisible, the differentiation having removed the small part of the dye that could make them distinguishable.

We have seen previously that, in the structure of any cell, the nucleus is necessarily held in a fixed position by its solid articulation with the balls of the central dumbbells of the cytoplasmic network, and that the nucleolus is held in a fixed place in the nucleus by its articulation with the dumbbells of the nuclear network (reticular type) or with the central balls of the radiating dumbbells (euchromocentric type).

In this last type, the radiating dumbbell, the existence of which was previously unknown, comprises two balls and a rod, one of the balls being located in the nucleolus and at its periphery, the other attached to the internal face of the nuclear membrane (euchromocentric).

Therefore, when we see a nucleus such as that of the prophase nucleus of *Acacia scorpioïde*s (**Ghimpu**, 1930, fig. 140 of **Guilliermond's** *Traité de Cytologie végétale*) showing 23 peripheral euchromocenters and a large nucleolus in the center, we must conclude either that the fixative has caused serious alterations to the radiating dumbbells of which we see no trace, or that the rod of these dumbbells has dissolved normally in the nuclear background. However, this last alternative cannot possibly take place, because there must exist a single general law for the formation of chromosomes and this law includes, in reticulated nuclei, the conservation, throughout the course of kinesis, of the dumbbells constituting the substratum of the chromosome.

We must therefore conclude from this that there has been a profound alteration of the dumbbells, and that their rod has been destroyed in the case I have just cited. Let us recall, moreover, that this alteration, by fixatives, of the constituent elements of interphase or quiescent nuclei, has already been clearly demonstrated in the previous chapter.

Since both the cell nucleus and the nucleolus need to be held in place, otherwise they would fall by their own weight against the cell membrane or the nuclear membrane, and since, in fact, the support apparatus does exist and is the radiating cytoplasmic or nuclear dumbbells, it must be concluded that, *in any nucleus of the euchromocentric type possessing a nucleolus, the latter is held in place by radiating dumbbells and that these are, in interphase, the true prochromosomes of which the euchromocenters are only the external ball.*

However, neither in interphase nor in prophase have the many observers who have described the kinesis of non-reticulated nuclei observed prochromosomes or dumbbell shaped chromocenters, nor have they depicted them. **Jenny Doutreligne** clearly describes, in euchromocentric nuclei in interphase or prophase, the outlines of chromosomes (euchromocenters) that are slightly elongated and ''that present a slight transverse constriction that gives them the appearance of small dumbbells.''

This form, whose rod remains invisible, cannot be taken for a true dumbbell, but rather for a short rod with a partial constriction; it is very different from the radiating dumbbells, whose rod is always clear and often quite long. The smallness of the chromosome sketches contained in the interphase or prophase nuclei of all the species studied by **Jenny Doutreligne**, compared to the size of the same elements in the resting nuclei, indicates that there is something abnormal there, probably due to the alterations caused by the fixative. Other observations by the same author support this observation:

1. In figures 8, 9 and 62 of **Jenny Doutreligne's** thesis relating to *Luffa* and *Calycanthus floridus*, we see in metaphase two chromosomes carrying a satellite; we will see later that the satellites are dumbbells originating from the nuclear network in the reticulated nuclei and forming part of the substratum or reticulated band of the chromosome. This proves that in metaphase the chromosomes of *Luffa* have a framework or substratum formed by dumbbells with long, clean rods;

2. These same chromosomes are very long compared to the size of the euchromocenters that formed them and these must have given rise to several dumbbells articulated end to end for them to be provided with a satellite dumbbell;

3. In certain telophase figures or those of the transition to interphase, we clearly see the outline of a system of radiating dumbbells (figs. 69, 72, *Calycanthus floridus*; 136, 221, *Pastinaca sativa*) still covered with achromatic material;

4. Jenny Doutreligne observed that the nucleolus of *Calycanthus floridus* (13) "*contains in its peripheral region small enclaves which, by the **Feulgen** nucleal reaction, take on the same red tint as the euchromocenters*."

These chromatin enclaves are obviously the central balls of the radiating dumbbells of the interphase; they prove that the structure constituted by the radiating dumbbells must indeed exist in the interphase nucleus of the *Calycanthus* and that, consequently, the rod was altered by the fixative, its chromatin destroyed and its achromatic substratum destroyed or only altered, but become incapable of fixing the iron lacquer of hematoxylin.

The chromatin enclaves, central balls of the radiating dumbbells, are certainly released when the nucleolus decreases in volume; we do not know what happens to them at this moment;

5. Grégoire, then **Jenny Doutreligne**, showed that the beginning of chromosome formation in prophase is manifested by the appearance at two opposite points of each highly chromatic euchromocenter of an extension consisting of short achromatic cords so barely visible "*that they only reveal themselves following careful observation.*" These extensions subsequently lengthen and then become chromatinized progressively. This progressive formation of chromosomes can only take place by one of the following two processes:

1. Either by budding and progressive elongation of the euchromocenters;

2. Or by deposition of achromatic matter then of chromatic matter on pre-existing filaments or cords emanating from the euchromocenters.

As certain chromosomes of the euchromocentric type have satellites, it must be concluded that their substratum, their armature, is constituted by a series of several dumbbells articulated end to end by their balls, the chromosomes not provided with satellites certainly having the same constitution.

This chain of dumbbells, in which the euchromocenter occupies the central part, should have its two ends invisible at the beginning of prophase and it is on these that the achromatic and chromatic matters which will constitute the chromosome would be deposited.

This explanation is only incompletely satisfactory because the euchromocenter that occupies the middle part of the chromosome should actually be a dumbbell, whereas it is only a small, constricted or bilobed chromatic mass and not a true dumbbell with a clear rod connecting its two balls. The mode of formation of the chromosome therefore remains obscure and unexplained.

I looked for this mode of formation in the radicle of *Cucurbita maxima*; from my observations, I have not gathered anything that sheds light on it. In this species of *Cucurbitaceae*, I have not observed the achromatic extensions. This absence of achromatic extensions is confirmed by the fact that in metaphase the chromosomes retained their spherical or bilobed shape.

However, in only one nucleus out of several thousand examined, the chromosomes clearly had the shape of short, regular rods, three to four times longer than they were wide. Nothing in the other nuclei explained this fact. In this case, the radicle was fixed by 2% sucrose **Ringer's** fluid and 10% formalin of formic aldehyde.

Summarizing these observations, I must conclude that the mode of chromosome formation in euchromocentric nuclei remains unexplained because, the existence of a nucleolus in these nuclei implying the existence of a structure that maintains it in a fixed position and this structure being constituted by dumbbells, the absence of these in interphase and prophase implies:

1. Or a profound alteration of the nucleus's formed elements by the fixative, an alteration that makes exact observation of the phenomena impossible;

2. Or the normal, natural existence, during kinesis and from interphase onwards, of profound modifications of the nuclear structure that make it and the form of the elements that constitute it very different from those they present in resting or quiescent nuclei.

Currently, this second explanation could only be accepted if it were demonstrated that the fixative liquids do not cause the alteration of the nucleus' elements. Now this alteration has been formally proven.

These facts therefore establish that the drawings currently representing the structure of the euchromocentric nucleus and the shape of the euchromocenters at the beginning of interphase and in prophase relate exclusively to nuclei in which the real structure was destroyed or profoundly modified by the altering action of the fixatives.

Evidence of this destruction is found in **Jenny Doutreligne's** drawing of the interphase nucleus of *Pastinaca sativa* (14, fig. 124), which shows neither a chromocenter nor a network. Therefore, whatever the structure of the *Pastinaca* nucleus, it has been destroyed by the fixative.

The drawings of telophase nuclei (fig. 136, hematoxylin, and fig. 121, **Schoede** col.) appear to indicate the reconstitution of a radiating structure, but one which, it seems, based on the position of the chromatic granules of certain chromosomes, would have two rows of dumbbells.

Moreover, judging by the following passage from **Jenny Doutreligne's** thesis, it seems that she doubted whether the small euchromocenters she represented in interphase and prophase in certain species corresponded completely to the chromosomes of the preceding metaphase and interphase, or even to the actual shape of the euchromocenters. Indeed, we read on page 43:

*The interphase nuclei of **Luffa** therefore do not present a reticulated organization analogous to that which is well established elsewhere and, indeed, one cannot discern, in the achromatic body, any organization. This is what we want to express by saying that the achromatic body is "homogeneous; we do not in any way mean by this that this body*

would be truly homogeneous, or, to be more precise, that nothing there would represent chromosomal bodies persisting in their distinct individuality; we will see, on the contrary, that everything leads us to admit that the chromosomal individuals persist there and that, consequently, this achromatic background, apparently homogeneous, is, in reality structured.

We can conclude from these facts that, for euchromocentric nuclei at least, it is impossible to admit that the euchromocenters of the interphase are preserved unchanged until the following interphase, firstly because the euchromocenter considered until now is only a part of the real chromosomal element which is a dumbbell and because we currently do not know how it evolves during kinesis; and secondly because we currently do not know the real constitution of the chromatic substratum of the metaphase chromosomes of euchromocentric nuclei and how this substratum behaves during their division. These are phenomena which need to be determined entirely.
In the memoir by **P. Dangeard** (11 A) analyzed previously, we can read, page 371:

*Chromocenters become invisible in certain nuclei, depending on their condition or the nature of their fixation. **Regaud's** fixative, for example, is very poorly suited to highlighting chromocenters. Thus, in white lupin, with this fixative, the nuclei have a homogeneous-looking nucleoplasm, but the chromocenters that should be found applied against the nuclear membrane are invisible. This absence of chromocenters in nuclei fixed with **Regaud** is quite common; it is found in castor oil plant, beans, melons, etc. However, the chromocenters have not been dissolved by the fixative because they can be stained using the **Feulgen** method; they are only very sensitive to decolorization during regression using the ordinary **Haydenhain** hematoxylin method.*

*It is not only after using **Regaud** that nuclei appear devoid of chromocenters. We have noted the same fact following **Helly** or **Benda-Mèves** fixation. It is difficult to know what these homogeneous nuclei without chromocenters correspond to; it seems that they are most often nuclei at definitive rest. It is not excluded that the chromocenters may lose a large part of their chromaticity during the resting period of interphase, only to reappear in the early prophase stages.*

The observations we have made on the destruction by fixatives of the cytoplasmic network and the dumbbells that constitute it demonstrate that these bodies first destroy the chromatic substances and thus remove the affinity for the dyes from the figured elements, then gradually attack and destroy the achromatic matter; they thus succeed in destroying and completely eliminating the cytoplasmic dumbbells.

There can be no doubt that the action of the fixatives is the same on the figurative elements of the kernel. In the kernels of figure 3, plate 41 of this book, this destructive

action is evident, without it being able to be contested, by the total or partial disappearance of the rods of the dumbbells; for these the destruction cannot be in doubt since portions of them remain. The kernels of both figures 10 and 11 (Pisum sativum) of this same plate 41 are very demonstrative in this respect (**P. Dangeard**, fig. 27, 28, pl. 36).

This destructive action always occurs on the nucleus and with all fixatives without distinction, but to varying degrees. It appears, as **P. Dangeard** points out, that nuclei at rest or in interphase and even in the early stages of prophase are more sensitive to this altering action than nuclei in a more advanced state of kinesis, but it is nevertheless exerted on the latter and can cause the formation of satellites, constrictions, sectioning, and even complete splitting of chromosomes into several parts that can then be taken for normal chromosomes. We can therefore conclude that, until a method of fixation or examination has been established that rigorously respects the form and constitution of the figurative elements that constitute the nucleus, it will be very difficult, if not impossible, to determine the exact phenomena that follow one another during kinesis.

Formation of chromosomes in nuclei of the reticulate type. Nature and origin of chromonematic filaments.

Grégoire and **Wygaerts** (1903, 1904) demonstrated the formation of chromosomes by the presence of alveolated filamentous bands in prophase in *Trillium grandiflorum,* bands which would be identical to those of the telophase of the previous kinesis.

In 1906, **Grégoire** described two modalities of chromosome formation; in *Trillium grandiflorum* as in *Aillum*, the band retains, in a first modality, an alveolar structure until it has become a definitive chromosomal ribbon; in a second, the band "*seems to unfold into a thin, very elongated filament with a zigzag outline, as if it were made up of a series of small circular arcs placed end to end."*

In 1913, he specified that, in *Trillium, Galtonia*, and *Aillum*, the bands evolve into zigzag filaments by increasingly regular distribution of the chromosomal substance along certain longitudinal and transverse spans of the primitive structure of the reticulated bands.

Sharp (1914) and **De Smet** (1914) confirmed these facts, the first in *Vicia Faba* and *Marsilea quadrifolia*, the second in *Crepis virens*.

It is to these longitudinal and transverse spans described by **Grégoire** since 1904 that **Wejdowsky** (1912) gave the name chromonema.

Later, **De Litardière** (43) confirmed **Grégoire's** observations in the *Filicinae,* and particularly in *Hymenophyllum tunbridgense*, and showed with the greatest clarity the forma-

tion of chromosome bands and their various modes of evolution, but without perceiving the nature and origin of the granulations or nodules carried by the filaments of the chromo some bands.

P. Martens, in a long study on *Paris quadrifolia* (47), presented very precise and detailed observations on the evolution of the two constituents of the chromosome, the chromonematic part and the achromatic part, during the complete course of kinesis. These observations fully confirm those of **Grégoire** and provide new information on the evolution that precedes and follows division.

Note that **P. Martens** states that there is no doubt that the double longitudinal row entanglements of the chromonematic filaments, which many authors have considered to be chromomeres, really exist. But he concludes that these longitudinal row entanglements are new formations made at the expense of the spans (fig. VII, VIII, pl. IV). Now this interpretation does not agree with the real constitution of the chromosome bands at the moment of their release by dislocation from the nuclear network of the interphase. This constitution is clearly demonstrated in figure 12 of **De Litardière's** memoir representing, according to him, "*the details of the alveolated bands at the interphase stage*" in *Hymenophyllum tunbridgense*; it also emerges from figure 17; these two figures show that, from the beginning of the prophase, the chromosome band is constituted by two longitudinal filaments carrying a series of impasto con-nected from one filament to the other by transverse spans.

This same demonstration is given by numerous other figures in **De Litardière's** memoir, for example by figures 60 and 61 showing that, at an advanced stage of prophase in *Osmunda cinnamomea* (fig. 61), the alveolated chromosome band has a structure identical to that of the chromosome bands of the interphase nucleus, and that, in both cases, the band comprises two lateral filaments bearing string-like impingements connected from one filament to the other by transverse spans.

Examination of these various figures as well as those in **P. Martens'** memoir shows that, quite clearly, the filaments and spans of the chromosome bands at the beginning of prophase become the chromonematic filaments and spans of the formed chromosome.

A careful examination of the figures in **P. Martens's** thesis shows that, from the beginning to the end of kinesis, the portion of the nuclear network that constitutes the chromosome is made up of a series of polyhedral spaces placed end to end and limited by polygonal meshes whose sides are obviously formed by dumbbells of the initial nuclear network and which are at the same time the chromonematic filaments and their transverse spans.

In the chromosome, as in the nuclear network in interphase, the cavities corresponding to these polyhedral spaces are virtual, because their faces are open and represented only by their perimeter constituted by the dumbbells of the nuclear network.

It is therefore not strictly speaking chromonematic filaments, and even less a single chromonematic filament, spiral or not, which form the framework of the chromosome, but the entire dumbbell framework of the polyhedral spaces. It is these spaces and this framework which are filled by the achromatic matter (matrix); the dumbbell framework takes on various aspects depending on its degree of impregnation by the dye, a degree which depends on the altering action of the fixative, the latter irregularly diminishing the affinity of the dumbbells for the dye and going as far as their total dissolution, a fact which explains why new observers, not having been able to see the chromonematic filaments, dispute their existence.

In summary, it does not appear that there can be formation in the chromosome of a second chromatic filament before division, nor any real new entanglements in rows in the two filaments since they already exist, even in interphase. There can only be at this moment, before chromosome division, a deposit of new substance on the groups of dumbbell balls (chromocenters or chromomeres), a deposit which must also take place on the rods of the dumbbells (spans), which gives the impression that t*he original entanglements in series often lengthen until they abut, as P. Martens observed.*

But it is quite different after the longitudinal cracking into two parts of the achromatic material (matrix) which fills the meshes of the chromosome ribbon to give it the shape of a flattened sausage (**P. Martens**, fig. X to XIII pl. IV). In this case, after the separation of the two parts, each of them has only carried one of the lateral filaments with chromomeres; the comparison of figure XV of the same plate with figure VIII shows that, to arrive at the constitution represented in figure XVI, where we see the reappearance of two continuous lateral filaments which do not exist in the Figure XV, where only one continuous zigzag filament can be seen, it was necessary that at least one new filament be created, and this by the formation of new dumbbells.

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This creation appears necessary and even inevitable by the fact that, by the division into two parts of each chromosome and the distribution of each of them into two new nuclei, these each contain only half of the nuclear network of the parent nucleus. It is therefore necessary, in order for a network similar to that of the parent nucleus to be reconstituted in the daughter nuclei, that they each double, by a new formation.

The proof of the dumbbell shape of the elements that constitute the chromonematic filaments or their spans is provided by the dumbbell shape of the satellites or by the dumbbell shape of the link that joins the two parts of a truncated chromosome. When the

fissuring of the chromosome is complete, the spans that join the two halves in all directions are the dumbbells of the polygonal meshes of the polyhedra; at the moment of the separation of these two halves, it must take place, very probably, not by scission of the rods, but by disarticulation of their balls which separate, and it is probably these disjointed balls which must be the starting point for the formation of new dumbbells in a number roughly equal to those which have been carried along by the other half.

Comparing the drawings of the nuclear network in **De Litardière's** memoir in *Hymenophyllum tunbridgense,* we see that in telophase it has returned to the state it was in before the start of prophase, which proves that in telophase, half of the network of each daughter nucleus has formed an equal quantity of dumbbells, that is, new network. The schematic drawings of **P. Martens** (47 pl. IV) also show that at the end of telophase or in interphase, the chromosome has returned (XVI, XVII, XVIII) to the state it was in before the fissuring and then in the previous interphase (I, VIII, IX).

It is therefore a half-new network, half of whose dumbbells are newly formed, which exists at each interphase, and not a network whose dumbbells would be entirely those of the network of the previous interphase, something impossible since half of these have passed into each of the two daughter nuclei.

We must therefore conclude that after the division of the chromosome into two parts, each daughter chromosome contains only half of the original dumbbells to which is joined an equal quantity of newly formed dumbbells. Consequently, neither the chromosomes nor the nuclear network remain unchanged in successive interphases.

This fact opposes the notion of the indefinite permanence of chromosomes, their morphological identity, and their individuality in the successive mitoses of the nucleus. Only half of the chromosome comes from the parent, or initial, chromosome; the other half is newly formed. It follows that after four successive interphases, a nucleus no longer contains more than the sixteenth part of the nuclear network of the first unchanged. Thus, the preceding presentation explains the nature and origin of the chromonema and proves that it is made up of the dumbbells and dumbbell filaments of the interphase nuclear network. The following presentations concerning satellites and chromomeres will provide further evidence of this nature and origin.

Eichorn (16) (1931) disputed the existence of the chromonema based on the following facts:

1. The chromonema is an artificial structure due to the action of fixatives; the proof is that with good fixatives it is not seen, and that it is made to appear by using **Flemming's** liquid and, even better, **Perenyi's** liquid (alcohol, chromic acid, nitric acid).

This fact in no way proves that the chromonema does not exist. It is quite possible that these liquids constitute a good means for making the chromonema appear in destroying substances that mask it. This is how **Kuhne** and **Ewald** demonstrated the neurokeratin network of myelinated nerve fibers by treating the fresh nerve with absolute alcohol for 24 hours, then with boiling alcohol for 2 hours, then with ether for 24 hours, which allowed them to observe that neither potash, nor sulfuric acid, nor even digestion with pancreatin destroy this network.

Now the chromonema is a network of dumbbells that has a constitution almost identical to that of the nuclear network, that is, of the portion of this network that constitutes the chromosome.

It would therefore be no surprise if **Perenyl** liquid were favorable to the demonstration of the chromonema.

2. The chromonema theory would not be satisfactory because it takes no account of the existence of satellites and does not appear to be capable of explaining them.

Let us first note that this is not a theory or a hypothesis, but a figurative, material, visible object existing in the chromosome. The question, which is not at all theoretical, is only to know whether this object exists or not, and whether or not it is an artifact.

As for the relationship of the chromonema with the satellites, we will see in the next chapter that the nature and origin of these are precisely explained by the origin and nature of the chromonematic filaments of which they are the visible extremity. The chromonema also explains the nature of the filament that connects the two sections of a chromosome. The other objections formulated by **Eichorn** have no direct relation to the only question at stake here: the existence of the chromonema, its nature, and its origin.

Nature and significance of satellites

Satellites are appendages, one or two in number, located at the end of a chromosome and formed either by a granulation connected to it by a thin filament, or by two granulations linked together by this filament.

The satellites, discovered in 1912 by **Navachine** in *Galtonia candicans*, can present numerous variants that **Guilliermond** brought together in a very suggestive way in figure 136 of his treatise on *Plant Cytology* which shows the different forms observed in various plants. This figure, reproduced here **(fig. 2),** shows, among other significant facts, that the satellite is a dumbbell organelle, that it can be formed by two dumbbells articu-

lated end to end (2) or by two dumbbells placed side by side **(*x*)**, or even that the chromosome can be constituted exclusively (***g)*** by two dumbbells placed side by side. In this figure, we see:

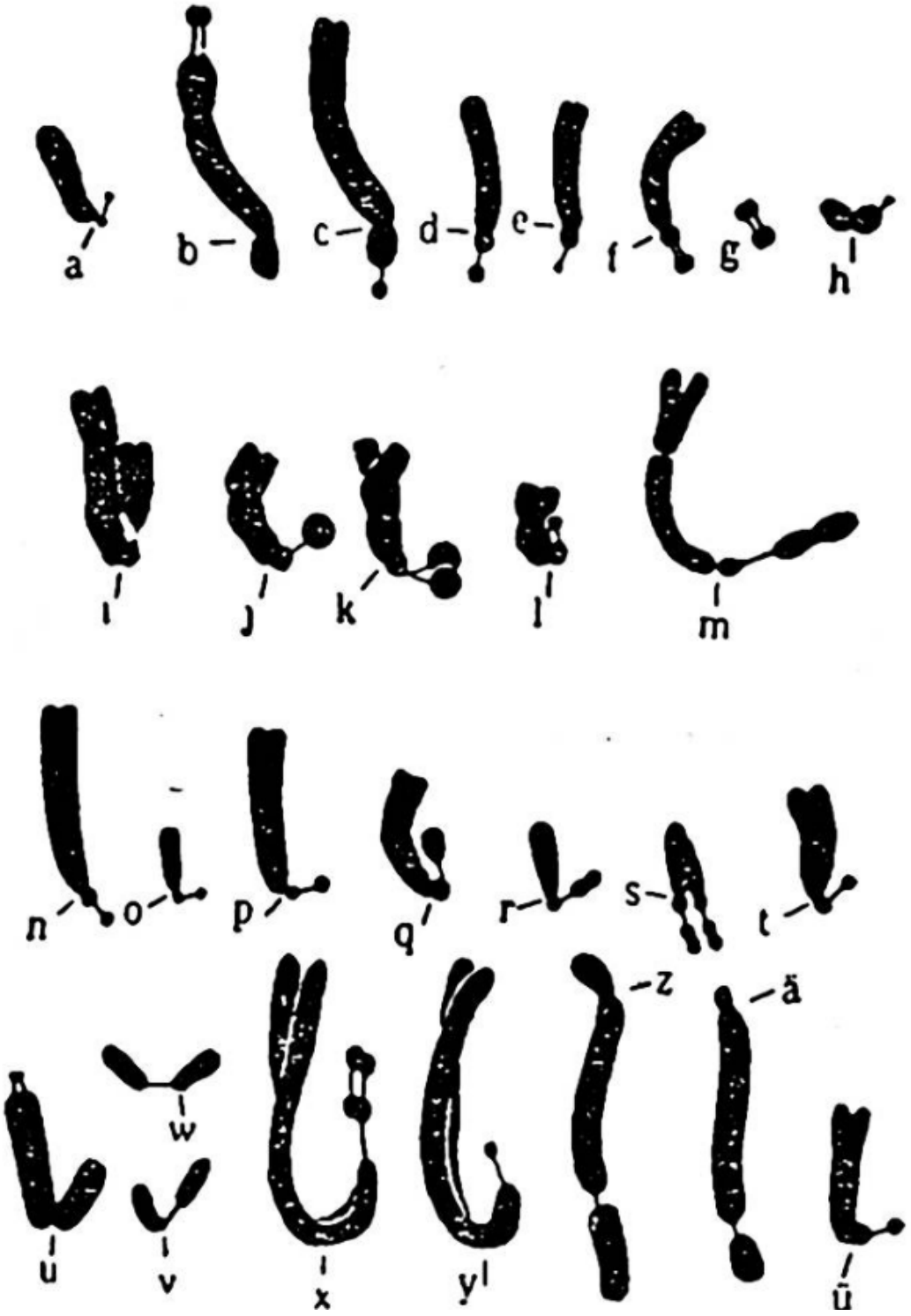

Fig. 2.

a, *Galtonia candicans (**Navachine**); **b à h**, Muscari; **i à m**, Ornithogalum**(Delaunay)**; **n à s**, Crépis (**Navachine**); **u**, Bellevalia forficulata (**Delaunay**); **v à w**, Colchicum umbrosum; **x à y**, Aillum cepa; **z à**, Fritillaria impérialis (**Taylor**); **ū**, Ranunculus acris (**H. Sorokina**).*

1. Chromosomes extended by a complete dumbbell, ***c, d, e, j, n, o, p, q, t, u,*** at only one of their ends;

2. Chromosomes extended by a complete dumbbell to the terminal ball of which the ball of a neighboring dumbbell detached from its rod is still adhered, ***m, s***;

3. A chromosome formed solely by two dumbbells placed side by side, ***g***;

4. Chromosomes bearing at their end two dumbbells placed side by side, ***b, f, k, s***;

5. A chromosome extended by two complete dumbbells placed end to end, a third dumbbell being attached laterally to the terminal dumbbell, ***x;***

6. A cracked chromosome ***s,*** each half of which carries at its end a dumbbell to which is adhered a ball of the neighboring dumbbell whose rod has been broken/destroyed;

7. A chromosome ***a*** carrying a dumbbell at one of its ends and a ball at the other, the whole coated with the same chromatic material deposited on the body of the chromosome;

8. Several chromosomes separated into two sections by the rod of a dumbbell, ***v, W, z***;

9. A chromosome ***u*** carrying at one end two dumbbells of which only the terminal ball and part of the rod are visible.

This examination clearly shows that the satellites are in reality the visible extremities of the dumbbell filaments detached from the nuclear network which constitute the chromosome bands at the beginning of prophase and which become the chromonematic filaments when the achromatic substance (matrix) is deposited on them. The dumbbells which some chromosomes reveal at their extremities are therefore those of the extremities of the chromonematic filaments which have become visible either because the fixative has destroyed the achromatic matter (matrix) there, or because it has disappeared prematurely, as normally happens for the entire chromosome at the end of telophase. But it is very likely that only the altering action of the fixative is at play here, the proof of this action being provided by certain particularities of the chromosomes explained later.

According to some observers (**Baranov, Madame Senjaninova, Mademoiselle H.Sorokina, Delaunay**), satellites are granules adhering to the nucleolus, at the periphery of which they form a slight protuberance and which, during prophase, attach to the chromosome that carries them.

Other authors have contested the presence of satellites at the periphery of the nucleolus at rest, and some (**Eichorn**, 1931) even deny the existence of any protuberance.

Let us recall that plates 38 and 39 demonstrate beyond any doubt the existence of a layer of granules which, upon becoming visible, form at the periphery of the nucleolus a complete crown of protuberances which are the central balls of the radiating dumbbells that attach to it and constitute its peripheral zone; this arrangement exists in both the euchromocentric and reticulated nuclear types.

Some portions of the network that will become chromosomes include one or two of the radiating dumbbells of the nucleolus; it is possible that the deposition of achromatic material on the chromosome occurs before the detachment of these radiating dumbbells, which remain bare. It would therefore be possible, although nothing supports this hypothesis, that some satellites are the radiating dumbbells that were attached to the nucleolus.

But this explanation is no longer valid in the case where the chromosome bears at its end two dumbbells joined end to end by their balls. Indeed, this presence of two complete or incomplete satellite dumbbells joined end to end (***r, s, x,*** fig. 2) demonstrates that it is certain that another mechanism causes them to appear. This results from the fact that some satellite dumbbells have their balls still coated with achromatic material deposited on them (***j, k, m, q,*** fig. 2), the absence of this material on their rod proving that in this place it was probably dissolved by the fixative.

The sectioning of chromosomes such as ***z, a,*** figure 2, is the manifestation of this same action localized to the region of the rod which connects the two sections. It seems obvious that the cause which makes the satellite dumbbell visible is the same as that which, causing the sectioning (cutting), makes appear only a dumbbell rod connecting the two sections; the proof of this is also provided by chromosome ***i,*** figure 2, the sectioning of which has revealed an almost complete dumbbell, one of its balls being totally visible, the other half.

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In some chromosomes, the dissolving action of the fixative is limited to a simple appearance of constriction or constriction at one or two points; when it exists at two points, as in chromosome ***b*****,** Figure 2, there is no reason to think that one of these points is rather than the other the place of insertion of the chromosome on the spindle as is admitted; it is evidently the same cause which has determined the two constrictions and it thus appears that neither one nor the other is caused with a view to an insertion of the chromosome, nor by this insertion itself. This fact appears clearly in chromosomes ***i*** and ***m*****,** Figure 2 where the author (**Delaunay**) has fixed the constriction at points ***i*** and ***m,*** between two dumbbell balls, that is to say at a point where the dissolution of the matrix by the fixative is clearly taking place. The same indications are provided by the ***z*** and ***ä*** chromosomes, fig. 2, showing two points of dissolution of the matrix, one where it only begins and has only caused a constriction.

It seems certain, according to these observations, that the points of constriction of the chromosomes have no relation to their insertion on the spindle and are only the points where the attack of the matrix by the fixative begins.

It is clear that the more or less pronounced constriction of the chromosomes, the complete sectioning not revealing any part of a dumbbell (***m,*** fig. **2**), the sectioning exposing the rod of a dumbbell connecting the two sections (***v, w, z, ä,*** fig. **2**), the sectioning revealing a complete dumbbell with its two balls (***i,*** fig. **2**), are not morphological peculiarities of the chromosome, but only successive stages in the dissolution of the achromatic matter (matrix) by the fixative.

These peculiarities certainly do not exist in normal, living chromosomes, and the cause that provokes them is the same as that which makes the satellite dumbbells visible, these probably remaining hidden in the matrix of living chromosomes, as in the matrix of fixed chromosomes, in general.

CLEAVAGE OF SATELLITES

It is stated in **Guilliermond's** treatise on *Plant Cytology* that "*when the chromosome divides, the cleavage line extends to the level of the satellite; it is thus divided into two equal halves and its pedicel fissured longitudinally.*"

This statement is certainly not consistent with the reality of the phenomena. First, the multiplication of dumbbells never takes place by longitudinal division; dumbbell filaments form and elongate: either by division of a dumbbell ball whose two parts move apart by stretching a rod between them, a second division of the primitive ball forming the second ball of the dumbbell; or by birth and elongation of the rod at a point on the formative ball where the chromatin accumulates, then subsequent formation of the two balls.

Moreover, in Figure 136 of this same treatise on cytology, we can see two unfissured chromosomes (***b, u,*** Fig. 2) bearing side by side two dumbbell satellites at one of their ends, while others (***y, t, ü***, Fig. 2) clearly fissured (***y***) or bearing the indication of fissuring ***(t, ü***) bear only one.

The presence of two satellites at the end of a chromosome does not therefore mean that they come from a longitudinally divided satellite, but only means that they are the end that has remained or become visible of two original dumbbell filaments that constituted the chromosome and which will be separated from each other by the fissuring.

In summary, satellites are the visible ends of the dumbbell filaments of the portion of the nuclear network (chromonematic filaments) constituting the framework of the

chromosome; it is the dissolution of the achromatic substance (matrix) by the fixative that makes the satellite dumbbells visible at the ends of the chromosomes and which also, by causing them to be cut, uncovers either a dumbbell rod or a complete dumbbell in the very body of the chromosome.

The points of constriction or constriction are only the beginning phase of the cutting and have no connection with the insertion of the chromosome on the spindle.

This nature of the satellites and the cause that causes them to appear remove all meaning from them, either for the identification of the chromosomes that are equipped with them, or for differentiating them from the others in terms of their genetic role, all having the same constitution. The same is true of constrictions and sectioning caused by the same cause of alteration.

Note further that if the fixative, after having caused the sectioning of the chromosome, also destroys the rod connecting the two sections, they become free and we will find an extra chromosome in the count, this one, like the other section, being smaller than the ordinary chromosomes. When there are two sectioned chromosomes, there are therefore 4 smaller chromosomes, including 2 additional and accidental ones; these two distort the count. The fixative can therefore delete chromosomes or chromocenters that it destroys, or create a surplus number by a total sectioning.

So, in summary, the long or medium chromosomes in the shape of flattened sausages are made up of a substratum or framework of dumbbell filaments whose points of articulation by their balls (chromomeres) are either the original chromocenters of the nuclear network, or original chromocenters of the nuclear network, or chromocenters of new formation. Proof of the existence of these dumbbells in the chromosomes can be found in Figure 268, Plate IX of the memoir by **De Litardière** already cited (43), showing a chromosome in which several dumbbells are distinguished, including a very clear one in the upper part.

These are the points of articulation of several dumbbell balls that **P. Martens** calls impasto; this name is faulty because an impasto is only a deposit of substance without a defined form and without morphological significance, while groups of 2 to 5 or 6 dumbbell balls have very precise shapes and morphological significance.

A characteristic example of impasto is the deposition of material, probably of nucleolar origin, which occurs during anaphase on chromosomes and gives them the appearance of clubs. **Jenny Doutreligne** (14), who revealed this fact, showed that the substance thus deposited does not stain by the **Feulgen** reaction, but stains red by **Schæde's** mixture (iodine green, basic fuchsin), while the body of the chromosome stains blue.

In reality, in the entanglements reported by **P. Martens**, there is indeed a deposit of added material on the filaments, but beneath this light deposit, there is a group of dumbbell balls which is not an entanglement.

Nature and Role of Chromomeres

The constitution of the chromosomes presented above is consistent with **Strasburger's** old concept, according to which the chromosome is formed by a substratum of linin filaments bearing chromatin granules. Later, it was seen that these, which were called chromomeres, can be formed by the union of several smaller chromatic granules called chromioles (**Eisen** 1900).

It is surprising that these first observations on chromomeres were considered erroneous or due to artificial structures created by fixatives or even to bends in the linin filaments, because it clearly appears that the chromomeres are nothing other than the chromocenters of the nuclear network carried along, during the dislocation of this network, with the linin filaments released by it; in reality, these filaments are formed by the dumbbells of the nuclear network articulated end to end.

It is because the chromomeres are the chromocenters of the nuclear network that they are, like these, made up of several granules (3 to 5 or 6) which are one of the balls of the same number of nuclear dumbbells. This fact is easy to verify because, from each chromocenter well preserved with the network, a number of dumbbell rods or linin filaments, equal to the number of balls that compose it, depart.

According to **Guilliermond** (*Traité de Cytologie végétale*, p. 237), there is a tendency to day to admit, with **De Litardière**, that *''the chromomeres have never existed, only in the imagination of certain cytologists*.''

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This opinion of **De Litardière** is all the more surprising since we find precisely in the plates of his memoir on the somatic karyokinesis of *Filicinae* (43) multiple examples of linine filaments or dumbbell filaments showing strings of chromomeres of the greatest clarity drawn by him in interphase or prophase. This will be seen for example in the figs: 11, 16, in *Alsophila phalerata*; 97, in *Oleandra articulata*; 115, 116, 117, 118, in *Polypodium vulgare*; 125, in *Polypodium Schneideri*; 131, in *Gleichenia flabellata*; 249, 250, 251, in *Dryopteris spinulosa*; we will see many others, which I have not indicated, in the plates of this memoir. On the other hand, we find the following passage on pages 401 and 402 of this memoir:

As I said about Hymenophyllum tunbridgence and Pteris cretica, if the swellings observed in the chromosomal elements at telophase appear at first sight to be strongly colored and

independent granules, connected by less chromatophilic tracts, especially in overly discolored preparations, which, incidentally, could suggest the idea of the existence of independent chromatic corpuscles fixed on a filament of a different nature, these aspects do not correspond to reality, since by varying the micrometric screw, even in these cases of lightly colored nuclei, we see the more colored clusters becoming attenuated to become one with the rest of the filament The chromosome appears, both when it is alveolated and when it is not, to be formed of a single substance, and obviously one cannot consider the thickest parts as consisting of chromatin and the thin parts, including the anastomoses that connect the various chromosomes, as made of linin.

The chromosomal elements of interphase offer the same constitution...

By concluding in the passage cited above that the swellings carried by the chromosomal filaments do not correspond to reality "*since by varying the micrometric screw we see the more colored clusters diminishing to become one with the rest of the filament*", **De Litardière** did not interpret his observation exactly because, in the swellings he observed, there was indeed a group of several granulations (chromocenter) covered by the deposit of a material colorable by hematoxylin which masked it; such a deposit always thins at the level of the filaments which leave it and which, being the dumbbell sticks, are the incontestable, formal proof of the existence, under the deposited material, of a group of dumbbell balls. The thinning of the deposited material on both sides of a bulge proves, contrary to **De Litardière's** conclusion, that there does indeed exist, at the center of the bulge, a figured, globular body, because this material is only deposited on figured objects.

The bulges in question are therefore indeed morphologically characterized elements, that is, chromomeres or groups of granulations which are dumbbell balls, each group being a chromocenter of the nuclear network of the interphase. **De Litardière's** observations therefore in no way demonstrate the non-existence of chromomeres as he asserts; on the contrary, they contribute to the demonstration of their existence.

Christine Hermans (31, 1936) observed, in a memoir on meiotic prophase in the anther of *Lilium martagon*, chromosomal filaments bearing numerous serial granulations, which she did not recognize as actually existing. Figs. 2, 3, and 5 of our plate 42 show these filaments. Figure 3 shows, on the left, a fragment of a nucleus at the leptotene stage; then on the right and in the center, two nuclei at the zygotene stage showing paired filaments but not yet brought together; according to **Christine Hermans** and in accordance with **Grégoire's** conception, the paired filaments at this stage do not come from the division of a single filament, but are two originally distinct filaments.

Figures 2 and 3 of our plate 42 show the chromosomes at the pachytene stage in the form of long filaments, along whose path we notice more colorful parts called chromo-

meres, which have the appearance of spherical or oval granules. According to **Christine Hermans**, these chromomeres do not actually exist and are more colorful points due to the accordion-like folding of the filaments at their level. She draws this conclusion from the fact that the **Feulgen** nucleal reaction uniformly colors the entire length of the filament and not just the chromomeres. This does not constitute sufficient proof because:

1. The accordion folding, as shown in Figures 15 (b), 16 (a, b), 17 (d, d'), in lateral view, is not such as to give rise to more colored points at the angles of the folding *because, at this level, the thickness of the substance of the filament, seen from the front, is not greater than at any other point located between the vertices of the angles;*

2. The folding is not such as to increase the thickness of the filaments at the angles, and yet the figures of **C. Hermans** represent the chromomeres with a thickness often more than double that of the filament. It is therefore indeed a globular body which exists at this level;

3. To demonstrate the non-existence of chromomeres, Christine Hermans indicates that *''the handling of the micrometric screw demonstrates that very often, in the filaments seen from the front, the so-called chromomeres actually extend by their two ends.''* **-** This is further proof of the real existence of chromomeres because these are the points of articulation of the balls of the dumbbells constituting the filament, and these balls are always articulated by at least two, their union forming an oval body with a major axis of a length twice the diameter of a ball when the dumbbells are properly articulated end to end. This is how **C. Hermans** represented the chromomeres at numerous points in plate 1 of his memoir, notably in figures 10, 15, 16 where they have exactly the shape of the dilations of the chondrioconts and the long filaments which result from the dislocation of the cytoplasmic network exposed in the first chapters of this book, dilations which are the points of articulation of the balls of the dumbbells which constitute these filaments. **-** These oval chromomeres also have the same shape as the varicosities of animal neurofibrils formed by the articulation of two dumbbell balls;

4. The chromomeres exist on all the filaments of the nucleus in figs. 11, 12, 13, 14 of pl. 1 of **C. Hermans'**s memoir, which would mean that all these filaments without exception are folded accordion-style along their entire length. **-** If this were the case, it is impossible that there would not be a certain number which appear laterally to show their folding. Now there is not one which appears this way.

The proof of the real existence of the chromomeres is clearly provided by figures 3, 4, 17, 19, 20, 21, plate 1 of **John Beal's** memoir (3) reproduced in figures 4, 5, 6 of our plate 41 and which show:

A) That, in heterotypic prophase, the chromosomes arise from the dislocation of the nuclear network clearly visible with its nodules at the corners of the meshes in Figure 4 of our plate 41, Figures 3 and 4 of J. Beal. In these two nuclei in Figure 4 (Figs. 3 and 4 of **John Beal**, leptotene stage on the left, passing to zygotene on the right according to the author), there is no doubt that this is indeed the normal nuclear network in the process of dislocation, and whose *nodal granules are chromocenters, that is to say, as we have previously demonstrated, the points of articulation of the balls of the dumbbells constituting this network, and which will later be the chromomeres.*

B) In figures 5, 6 of our plate 41 (figs. 17, 19, 20, from the same memoir by **John Beal**) the dissociated filaments present in several places various folds seen laterally, but which *nevertheless bear chromomeres at the angles* of the folds (fig. 5). On the other hand, the gemini shown on the right of our figure 6, plate 41 (no. 21 by **J. Beal**) bears balls which, clearly, are dumbbell balls and not attributable to folding. The examination of numbers 19 and 20 of this figure 6, plate 41, leads to the same conclusion;

5. The fact that, through the **Feulgen** nuclear reaction, the chromomeres no longer appear on the path of the filaments does not prove their non-existence because this reaction only stains the chromatin and the chromomeres may contain only a very small quantity. We know, in fact, that very often the balls of dumbbells do not stain or barely stain, even with hematoxylin and fuchsin. On the other hand, the chromatin could have been destroyed by the fixative, the altering action of which is manifested by a decrease or total loss of the affinity of the dumbbells for hematoxylin.

Thus, the existence of the chromomeres cannot be doubted. They are the points of articulation of the balls of the nuclear dumbbells; in the nuclear network of the interphase, they are the chromo centers.

P. Martens (47) states that there is no doubt that the double longitudinal row of chromonematic filaments, which many authors have considered to be chromomeres, do indeed exist. But he concludes that they are new formations made at the expense of the spans; this interpretation is at odds with the actual constitution of the chromosome bands and with that of the nuclear network. Other authors, notably **Eichorn** (15, 1931) have denied the existence of chromomeres. **Eichorn** says he has never been able to observe their existence except in one case (*Equisetum*) where fixation was manifestly poor (15, fig. 13, pl. 4). Now, in this case, the chromomeres he obtained are particularly in good condition and typical and denote satisfactory fixation; this was a normal structure and not abnormal as he indicates.

To prove the non-existence of chromomeres, **Eichorn** relies on:

1. The fact that this would be "*only a transient structure that can be observed only at the end of prophase and in metaphase since even at the beginning of prophase it is indistinguishable*."

This is an assertion contradicted by the facts, because most of the drawings in **De Litardière's** memoir show in interphase, and throughout prophase, filaments of the nuclear network, chromosomal bands bearing granules (balls of dumbbells) which are chromomeres and which, hidden but not disappeared in anaphase, reappear distinctly in telophase;

2. The fact that the presence of chromomeres would not be constant.

Now, in reticulated nuclei, it is a fragment of the nuclear network that constitutes the substratum or framework of the chromosome, and the chromomeres of this fragment are visibly the chromocenters of the interphase nuclear network. Therefore, as every nuclear network has chromocenters (groupings of dumbbell balls) at the corners of its meshes, without possible exception, it must be concluded that the chromosomal bands always carry them with them and that, consequently, the chromosomes always contain them, without exception, in the reticulated nuclei;

3. On the fact that no vital observation has so far confirmed their existence.

Everyone knows that vital observation does not allow the fine details of structure to be detected because of the very small difference in refractive power that exists between the formed elements and the environment that surrounds them. **Eichorn** concludes:

"*For all these reasons, we believe that the real existence of chromomeres has not been demonstrated and that, consequently, no general theory on the structure or composition of chromosomes can be based on these hypothetical formations.*'' I think, on the contrary, that the existence of chromomeres is now superabundantly demonstrated, as well as their origin and nature, especially after the facts set out in the three preceding chapters and even in this entire work.

I will say, in conclusion, that those who still have doubts about the existence of chromomere strings need only consult the figures of **Ramon y Cajal** to convince themselves; for example, those relating to the varicosities of the neurofibrils of the gray matter of the brain, which are chromomeres each consisting of two dumbbell balls. These formations are constant in all animals. A suggestive image of them can be found in figure 359 of **Ramon y Cajal's** treatise on the histology of nerve centers.

Counting chromocenters and chromosomes

The counting of chromocenters is not necessary in clearly reticulated nuclei if we assume that, in this type of nucleus, the substratum or initial framework of the chromosomes is constituted by a piece of network comprising a multiple number of meshes and consequently an equally multiple number of chromocenters, this word being reserved for the groups of dumbbell balls located at the corners of the meshes, groups also called impastos, or nodal granulations.

We will only deal here with the simplest nuclear type, the structure of which is constituted by a single row of radiating dumbbells or by a very simple network comprising only two rows of dumbbells, of which only the central row has its dumbbells in an exactly radiating direction. The type with a single row of dumbbells is the euchromocentric or prochromosome type.

It is accepted that, in this nuclear type, the number of chromosomes is substantially equal to the number of euchromocenters, and never exceeds it. The structure of this type being constituted by radiating dumbbells and the euchromocenter being only the peripheral ball of these dumbbells, the central and intranucleolar ball or the totality of each of these must become apparent at the beginning of prophase, at the moment when the dislocation of the nuclear structure occurs; the decrease in volume of the nucleolus during prophase seems to correspond well to the loss of its peripheral region constituted precisely by the central balls of the radiating dumbbells.

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We should therefore, at the time of the dislocation of the nuclear structure in prophase, observe the presence, in a non-reticulated nucleus, of either complete dumbbells or a large number of chromocenters or prochromosomes double that of interphase.

However, such observations have never been made, and the cause must be attributed to a serious alteration of the elements by the fixative since no author has reported, either in interphase, ase, or in nuclei at final rest, the existence of dumbbells or rods connecting the peripheral euchromocenters to the nucleolus, with the exception of **P. Dangeard** who, moreover, has neither seen nor indicated the significance and nature of these rods.

In both prophase and interphase, the dumbbell rods are destroyed by the fixatives; if they were not destroyed, the dumbbells would remain adherent to the nucleolus by their central ball or, if there had been dislocation of the nuclear structure, they would be apparent in their complete state and dispersed in the nucleus, except in the case where the disappearance of the dumbbell rods would be a normal phenomenon of prophase, a fact made very unlikely by the existence of dumbbell satellites at the end of certain chromosomes.

J. Doutreligne, who saw at the periphery of the nucleolus, in interphase and especially in prophase, inclusions of nuclear chromatin (colored red by the **Feulgen** nucleolus reaction) which are obviously the central balls of the radiating dumbbells, did not at the same time note the existence of the rod of these; she does not mention it either in her drawings or in her descriptions; it must therefore have disappeared due to the effect of the fixative since she did not see it in interphase.

The phenomena that occur during prophase are therefore very obscure, because the alterations produced by the fixatives eliminate the possibility of an exact observation of the real forms of the figurative elements of the nucleus and their respective relationships.

Currently, we do not know until what point the central balls of the radiating dumbbells remain adherent to the central portion of the nucleolus; they certainly leave it at a specific moment since the nucleolus generally disappears in metaphase. The fact that the rods of the dumbbells are destroyed in interphase by the fixatives also makes it impossible for us to know whether simple granules or complete dumbbells constitute the outline of the chromosomes in non-reticulated nuclei, whereas, as we have seen previously, it is filaments of complete dumbbells that constitute the outline of the chromosomes in reticulated nuclei.

An example which will clearly demonstrate our total ignorance of the mode of formation of chromosomes in nuclei is provided by one of the cases studied by **P. Dangeard** (11 A), that of the nuclei of the radicle of *Pisum sativum* in the aforementioned memoir. **-** In the quiescent nuclei 26, 28, 29, plate XXVI, we see, adherent to the nucleolus, the stumps (which **P. Dangeard** calls linear protuberances) of the rods of the radiating dumbbells *partly destroyed by the fixative with the chromocenter that the missing end carried*, since these nuclei do not contain any chromocenter. This destruction is even more complete in the quiescent nuclei of the pleroma and the periblema (13 to 17) and in the interphase nuclei 18, 19 in which we no longer notice any trace of rods or chromocenters; we only see one or two perinucleolar granulations (designated annex bodies or micronucleoli) in nuclei 13, 14, 19, bodies which appear here as the intranucleolar balls of radiating dumbbells, in the process of being released.

From these figures, compared to figures 23, 24, showing the formed chromosomes, one could deduce that all the nuclear dumbbells disappear by dissolution in the nuclear enchylem from the quiescent state and that during prophase the chromosomes are formed from scratch by this dissolved dumbbell matter. Now figures 25 and 26 show us cracked chromosomes, each half of which carries a dumbbell satellite, which demonstrates that each of them must be made up of 3 or 4 dumbbells articulated end to end by their balls. As observers have almost always observed that, in nuclei, whether reticulated or not,

each chromosome has as its basis for its formation either a portion of the nuclear network with its chromocenters, or only a chromocentre, it appears that it must be the same in *Pisum sativum*, and this all the more so since the quiescent nuclei 27 and 28, plate XXVI (**P. Dangeard,** 11 A) contain proof that others podded chromocenters that the fixative has destroyed; the observations of the first chapters of this work have established on the subject of the dumbbells of the cytoplasmic network this destructive action from which the nuclear dumbbells do not escape.

The observation of the phenomena of kinesis during the course of prophase, metaphase and anaphase is much less subject to error than during the end of telophase, during interphase and the beginning of prophase because it is during these last three periods that the formed elements of the nucleus have the maximum sensitivity to the altering action of the fixatives, a fact noted by various observers. As soon as the evolution of the chromosomes has begun it appears that a protection against the altering action of the fixatives is conferred on them by the deposition of achromatic matter on the elements which constitute their framework (portion of network and dumbbells), matter in which these disappear taking on the appearance of sausages.

These facts, which establish the profound alteration of the halteres and consequently of the chromocenters or prochromosomes by fixatives in resting, quiescent, or interphase and early prophase nuclei, demonstrate at the same time the errors to which the counting of chromocenters or prochromosomes in non-reticulated nuclei is subject; they explain the absence of fixity of this number in certain species.

Studying the number of chromosomes in *Phaseolus vulgaris*, **P. Dangeard** (11 B) wrote: *Other fixatives such as **Helly, Bouin-Hollande** and **Nawaschin** fluid show the existence in the bean of numerous chromocenters, almost all of which are peripheral and applied against the nuclear membrane. The nucleoplasm, after **Bouin-Hollande** or **Nawaschin** fixation, appears in the form of a poorly chromatic reticulum; in no case can the nucleus be said to be homogeneous following these fixations. We therefore do not agree with **Eichorn** who states that Phaseolus have nuclei devoid of a network, whatever the fixative used.*

It is obvious that the nuclei examined by **Eichorn** had their organization destroyed, because every nucleus has a nucleolus which is always maintained in a fixed position and is connected to the nuclear membrane by a network which, in the simplest case, is made up of a single row of radiating dumbbells whose peripheral ball pressed against the nuclear membrane is **Gregory's** euchromocenter and whose central ball remains hidden in the nucleolus. Therefore, the nuclear network was destroyed or rendered achromatic in **Eichorn's** preparations.

P. Dangeard also reports that in *Phaseolus vulgaris* there are 12 to 22 chromocenters, almost all of them peripheral and pressed against the nuclear membrane. However, in reticulated nuclei, the chromocenters are the impastoes which exist at the corners of the meshes of the network and the peripheral granules pressed against the nuclear membrane If there are 12 to 22 chromocenters in the nucleus, almost all of them peripheral, there would therefore be, for example, only at most 1/4 of non-peripheral chromocenters, that is to say from 3 to 6, a number too small for the existence of a true nuclear network to be possible or which indicates the existence of a nuclear organization with a single or at most two rows of radiating dumbbells. The small number of non-peripheral chromocenters therefore implies the conclusion that the existence of a true polygonal mesh network is impossible there or that, if it existed, this small number of non-peripheral chromocenters would be proof that most of them, that is to say the impastos of the angles of the meshes of the network, have been destroyed or made achromatic by the fixative. In any case, this destruction is made evident, whatever the nature of the chromocenters, by the variation in their number from 12 to 22, and by this remark by **P. Dangeard:**

First of all, we must note that some nuclei appear devoid of chromocenters in the meristem (of the radicle) and in the vicinity of other nuclei where these elements are very apparent. It is as if the chromocenters lost their colorability during a very short period of interphase, only to reappear with even greater evidence as prophase is more advanced. It is therefore in reality on nuclei already prophase or preprophase that we can count the chromocenters, because previously they are so small and so little colorable that they escape observation.

These last words, which establish the vulnerability of the chromocenters, demonstrate at the same time their profound alteration if we consider that they are also deprived of the dumbbell's net rod, which has been destroyed. This establishes that, in such cases, the counting of the chromocenters is illusory and inaccurate.

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For radish (*Raphanus sativus*), **P. Dangeard** (11 B) points out another cause of error. "*The numbers obtained in the meristem vary from 11 to 17,*" he writes, "*but there remains a certain uncertainty due to the fact that certain chromocenters are attached to the nucleolus and can be confused with accessory bodies of nucleolar origin*." I think that there are no accessory bodies of nucleolar origin and that they are always true chromocenters, that is, dumbbell balls, sometimes dechromatized by the fixative. But the main cause of error remains due to the fact, pointed out later by **P. Dangeard,** that certain chromocenters remain attached to the nucleolus by a less chromatic pedicle. Since this pedicle leads to the invisible intranucleolar dumbbell ball, what role should this play in the counting?

These facts establish the impossibility of an exact count of chromocenters. But even this is illusory because it does not correspond to the precise object that is the origin of the chromosome: the complete nuclear dumbbell or a group of dumbbells.

From these inexact counts results the fact that the numbers found are always lower than the diploid number of chromosomes, which is explained by the certain destruction of a part of the chromocenters.

Some observers, notably **P. Dangeard** (11 B), have sought to explain the superior number of chromosomes either by the fragmentation during prophase of certain chromocenters, for example in radish where **Guilliermond** and **Gautheret** observed in the nuclei that dumbbell-shaped chromocenters emerged which, for this reason, would be in the process of dividing. This hypothesis is inaccurate since the normal organelle of the nucleus is the dumbbell and not the chromocenter and the former does not result from a division of the latter.

Another equally unfounded hypothesis is that of **P. Dangeard** according to which, in the bean, it "appears that certain chromosomes can arise at the expense of the nuclear background, without the participation of a chromocenter." And this author adds:

It has not been demonstrated that, in nuclei of this type (bean and radish), the chromocenters are the same number as the chromosomes and that every chromosome is formed at the expense of a preserved chromocenter capable of being highlighted inside the interphase nucleus.

This demonstration is obviously not made, but another is made, that of the profound alteration and even the destruction of the chromocenters by the fixing liquids in the nuclei in interphase and prophase and this alone is sufficient to explain the inferiority of the number of chromocenters to that of the chromosomes.

Moreover, the prochromosome is certainly not the part which, in nuclei with a radiating structure, that is to say, euchromocentric and with one or two rows of dumbbells, forms the entire substratum of the chromosomes; it is probably complete dumbbells which form this substratum as in reticulated nuclei, and this because it is very likely that a phenomenon of the importance of kinesis is governed by general laws and does not occur with a large number of different modalities, these having been invoked above all by observers to legitimize dispositions recognizing as a cause the constant and very variable alterations caused by the fixing liquids to the organelles constituting the nucleus.

As long as the fixation of anatomical elements is not achieved without modification of their structure and without alteration of the organelles that constitute them, it will be

impossible to understand the currently very obscure phenomena that lead either to the reconstitution of the nucleus in telophase, or to the formation of chromosomes in prophase in the case of non-reticulated nuclei.

Annex bodies and micronucleoli

Many observers have noted the presence, against the nucleolus or in its immediate vicinity, of granules that some have called micronucleoli, others, accessory bodies.

Mrs. Eftimiu-Heim indicates the constant presence in cucurbits, against the nucleolus, of a granule that she considers to be a micronucleolus and which would present the same coloring affinities as the nucleolus, that is to say, would present like it a negative **Feulgen** reaction while hematoxylin colors it black.

P. Dangcard (11 A), who observed these granulations, one or two in number, in *Cucurbita maxima*, indicates that they turn bright red like chromocenters by the **Feulgen** reaction, while the nucleolus, uncolored, remains yellowish. Moreover, the same author, who noted the presence of these granulations in most of the plants he studied, almost always verified that they turn bright red by the **Feulgen** reaction; they are therefore indeed chromocenters. Ghimpu (1930) had already previously reported this fact in *Hordeum* and **Gavaudan** (1937) in *Lupinus albus*, *Cucumis melo*, and *Phoenix dactylifera.*

On the other hand, **P. Dangeard** (11 A) has reported and shown, in some drawings, granules of the same size very close to the nucleolus from which they are clearly separated by a certain space but to which they are connected by one of the filaments that he calls linear protuberances of the nucleolus. We will see them for example in *Lathrea clandestina* (fig. 22, pl. XXXI) in *Cyclanthera pedata* (fig. 6, pl. XXVII) *Elodea Canadensis* (fig. 24, pl. XXVII). These are obviously dumbbells constituting the first row of these organelles in a nucleus with a relatively uncomplicated radiating structure comprising only two, perhaps three rows of dumbbells, of which only those of the first row have exactly the direction of the radius of the nucleus the peripheral ball of each dumbbell of this first row being articulated with the central balls of two or three dumbbells of the second row and the set of these three or four balls constituting a chromocenter.

The granular bodies attached to the nucleolus are considered by some to be micro-nucleoli, and their role remains unknown. It has been hypothesized that they probably play a role in the course of kinesis, although it has not been specified which one.

According to **P. Dangeard** (11 A), "*when the nucleolus is displaced, it drags the accessory body with it, which can hardly be explained except by a material link between these two*

formations. It is therefore not excluded that the accessory body could be a sort of bud generated by the nucleolus."

The nature, origin, and role of these bodies or micronucleoli have been demonstrated:

1. By the microphotographs in plate 39 of this book showing that the entire peripheral zone of the nucleolus of *Elodea canadensis* is made up of granules which are the central balls of the nuclear radiating dumbbells. The peripheral rods and balls of these dumbbells are sufficiently marked in certain places in figures 1, 2, 4, 5, 6, plate 38, and in figs. 1 and 2, plate 39 so that no doubt can remain about the nature of these perinucleolar granules.

Note that one of these balls, located at the lower part of the nucleolus and outside it, corresponds to the accessory bodies or micronucleoli, but there is nothing to suggest that this granulation could be different from those that constitute the peripheral zone of the nucleolus; its position may be natural, although exceptional, but it may also have been brought there and removed from the periphery of the nucleolus by the razor. It is a central ball of a radiating dumbbell as well as the neighboring balls.

2. By the existence, around the nucleolus, of a serration or festoon, each tooth or projection of which represents the external edge of a perinucleolar granulation, that is to say, of a central ball of a radiating dumbbell. This festoon can be seen in figures 1, 2, 4, 5 of plate 38 and in the figures of plate 39, notably in figure 2. **P. Dangcard** depicted it on the periphery of the nucleolus of *Arum italicum*, in nucleus no. 8, plate XXXV of the afore mentioned memoir (11 A), but without noticing or mentioning the importance and significance of this device; in fact, he concluded that these protuberances *correspond*, in this case, *to micronucleoli generated by the main nucleolar body*;

3. By examining the drawings of certain authors, notably those of the nuclei of *Lathrea clandestina* (**P. Dangeard**) showing the rods of the radiating dumbbells connecting the nucleolus to the nuclear membrane, but whose central intranucleolar balls remain invisible;

4. By examining certain drawings of **P. Dangeard's** memoir (11 A, fig. 24, pl. XXVII) showing chromocenters located between the nuclear membrane and the nucleolus and connected to the latter by a rod which demonstrates that it is a dumbbell whose central ball has remained hidden in the nucleolus;

5. By the fact that these perinucleolar granules are formed by chromatin impregnating an achromatic substance, become colored bright red by the **Feulgen** reaction and thus present the characteristics of the balls of the dumbbells, that is to say of the chromocenters, and not those of the nucleolar substance;

6. By the fact that **J. Doutreligne** detected, on the periphery of the nucleolus, enclaves which turn bright red by the **Feulgen** reaction, which proves that they are the hidden central balls of the radiating dumbbells whose rods can be seen in the figures on plate 38 of this book, as well as in figures 15 to 35 of plate 31 of the aforementioned memoir by **P. Dangeard** (11 A).

These facts agree to demonstrate, beyond any doubt, that the micronucleoli or accessory bodies are the central balls of certain radiating dumbbells remaining hidden in the nucleolar substance and which become visible, either because they are in the process of being released by the prophase dislocation of the nuclear organization, or by the preparation maneuvers (notably by the razor) which carry them out of the nucleolus, or because a part of the nucleolar substance is destroyed or altered by the fixative at the periphery of the nucleolus.

The fact that some of these accessory bodies do not stain by the **Feulgen** reaction in any way invalidates this conclusion because their chromatin can be dissolved by the fixative; **P. Dangeard** has also noted that some of them do not stain by hematoxylin or by the **Feulgen** reaction, which clearly demonstrates that all chromatic matter has disappeared.

The purpose of these ancillary bodies is therefore that of the radiating dumbbells, of which they are the central ball. They are therefore not micronucleoli.

As for the linear protuberances of the nucleolus, which **P. Dangeard** considers to be formed of nucleolar substance because they do not stain red by the **Feulgen** reaction, it has been previously demonstrated that they are the rods of the radiating dumbbells. These rods are often devoid of chromatin, which is located in the balls of the dumbbell. In this case, the very slightly chromatic rod is made up solely of the achromatic matter that constitutes the substratum of the entire dumbbell and gives it its characteristic shape.

P. Dangeard has observed (11 A, p. 344), by the *Feulgen reaction, that the pedicle which connects the peripheral chromocenters of the nucleolus* in *Lathrea clandestina is an extension of the chromocentric substance*; as the linear protuberances of the nucleolus just discussed are a part of this pedicle which, in reality, is the rod of the radiating halteres, they must, like it, become colored red by the **Feulgen** nucleal reaction; when they do not take on this color, this therefore only means that they are altered by the fixative, which is also proven by the state of profound alteration of the nucleus as, for example, in the case of *Pisum sativum* cited above.

Unable to distinguish the role of these nuclear protuberances from the nucleolus, nor that of the pedicle connecting the chromocenters to the nucleolus, **P. Dangeard** writes (11 A, p. 373):

It is difficult to know what these relationships between nucleoli and nuclear chromatin mean. They probably respond to adhesion phenomena between the nucleolar substance and the chromatin rather than to an exchange of substance occurring between these two formations. However, we cannot affirm anything.

In reality, these relationships are very simple: the accessory bodies, micronucleoli, and rounded protuberances are the central balls of the radiating dumbbells, while the linear protuberances of the nucleolus and the pedicles of the chromocenters are the rods of these same dumbbells; the central balls of these have, with the nucleolar substance, only a contact relationship.

The nucleolus is therefore formed in large part by figurative elements, the central balls of the radiating dumbbells which occupy its peripheral zone and by the nucleolar substance in which they bathe, the constitution of the central part remaining unknown; it thus appears that the nucleolus appears to be constituted solely by these two different elements and represents the place of condensation of the nucleolar matter.

Classification of nuclear types

For some authors, there is a euchromocentric type, a reticulated type, and probably intermediate forms between these two main types (**Litardière**). **A. Eichorn** (15) distinguishes:

1. Non-reticulated nuclei with prochromosomes (Cochlearia type);

2. Nuclei without a network and with multiple chromocenters (Fatshedera type);

3. Reticulated nuclei with multiple chromocenters (Pinus type);

4. Nuclei comprising simply a regular network (Allium type).

To appreciate the value of the bases of this classification, we must remember from **A. Eichorn's** memoir:

1. That he considers prochromosomes as different formations and perfectly distinct from chromocenters;

2. That he bases this distinction exclusively on the fact that chromocenters are smaller than prochromosomes and that apart from this fact he indicates that the two categories of non-reticulated nuclei which contain them *have a great resemblance*;

3. That he indicates that nothing can allow us to think that the nucleolus participates in the construction of chromosomes.

The facts I have previously presented regarding the anatomical structure of the nucleus demonstrate that the classification proposed by **A. Eichorn** rests on inaccurate bases, not in accordance with this anatomical structure.

These facts establish in fact:

1. That there is no nucleus without a network. That this network, always present, is constituted, in its simplest form, by a single row of radiating dumbbells directly connecting the nuclear membrane to the nucleolus and, in its most complicated form, by a network of much more numerous dumbbells articulated together exclusively by their balls to form the meshes of the network, some of them having one of their balls articulated to the nuclear membrane, others a ball articulated with the nucleolus of which they constitute the peripheral zone. The latter have, like those of the simplest type, a radiating direction relative to the nucleolus.

Nuclei therefore differ from each other only by the complexity of the network that constitutes them or by the number of dumbbells constituting this network;

2. That, in the nucleus, every granulation is always one of the two balls of a dumbbell and must always, if the nucleus is intact, be placed at one end of its rod. The fact that this rod no longer exists is proof of a profound alteration caused by the fixative;

3. That in complicated networks, the sides of the polygonal meshes are formed by the rods of the dumbbells, the vertices of the angles of the polygons being occupied by the balls of the dumbbells articulated together at this point. In an intact nucleus, each corner vertex of these meshes is always occupied by a union of several dumbbell balls constituting the small chromatic mass called the chromocenter, the size of which varies according to the number of dumbbell balls that constitute it, a fact noted by various observers who have pointed out that the chromocenters comprise several distinct and joined granulations;

4. That, just as the cytoplasm has, among its functions, that of keeping the nucleus fixed and immobile in its central position, the nuclear radiating dumbbells or the nuclear network have among their functions that of keeping the nucleolus fixed in its position and also of maintaining the stability of the spherical shape of the nucleus;

5. *That the presence of a nucleolus in a nucleus necessarily implies the existence of a network of dumbbells interposed between it and the nuclear membrane.*

The existence of this network is all the more necessary since the balls of the nuclear dumbbells contribute to forming the nucleolus and constitute its peripheral zone.

Thus, all the chromatic masses visible in the nucleus, whether they are pressed against the nuclear membrane, or lodged in the space between the nuclear membrane and the nucleolus, or whether they constitute its peripheral zone, are dumbbell balls and are all identical in nature, origin, and constitution.

The various names chromocenters, euchromocenters, and prochromosomes that have been given to them are therefore not justified.

The nuclear types proposed by **A. Eichorn** therefore have no real existence and cannot be differentiated by the characteristics he indicated because:

1. Nuclei that appear homogeneous are those in which the dumbbells have been destroyed by the fixative;

2. Nuclei with a large number of chromocenters cannot be without a network, these being proof of the existence of the meshes of the network of which they constitute the nodal granulations;

3. Nuclei with a regular network without chromocenters cannot exist, because this network is formed by dumbbells, each of which necessarily includes two balls which, by articulating with those of other dumbbells, form chromocenters at the corners of the network's meshes. If these chromocenters remain invisible, it is because they have been altered and dechromatized by the fixative.

Let us examine, for example, the case of *Vicia faba* cited by **Eichorn** as showing a fine network devoid of chromocenters.

Sharp (1913) described in the quiescent nucleus of *Vicia faba* a network with a certain number of small karyosomes and several larger ones located around the nucleolus; in interphase nuclei there would exist only the network and small karyosomes.

According to **Schæde** (1925) and **Weber** (1927), the nuclei of *Vicia faba* are homogeneous, that is, without structure. **Heitz** (1931) describes peripheral and perinucleolar chromocenters.

For **Eichorn** (15), the nucleus of *Vicia faba* contains, apart from the nucleolus, only a fine network without chromocenters.

On the other hand, **P. Dangeard** (11 A) indicates the presence of chromocenters stainable by the **Feulgen** reaction (8 to 13) in the interphase nuclei, but does not include a network in his drawings; he described these nuclei as finely granular and drew them as such. *Wanting to observe what the effect of Regaud's fixative would be on nuclei well known for their reticulated structure*, he writes (11 A, p. 310):

The nuclei of Vicia faba (radicle) appear very finely granular during interkinesis; at no time can they be considered homogeneous (fig. 1, 2, pl. XXVI). More stainable portions are very visible elsewhere within the fine chromatin reticulum; they have the form of very chromophilic crystalloids. These are undoubtedly the elements called by some karyosomes, by others chromocenters...; they give a positive result by the ***Feulgen*** *method.*

It is obviously the alterations caused by the fixing reagents to the figured elements that constitute the nuclei that are the cause of the diversity of the results just listed. These alterations consist either of a complete destruction of the elements, or a partial destruction, or a modification that causes them to lose the power to fix the dyes.

The photographs in plate 75 leave no doubt in this regard. They represent the nuclei of a sprouted bean radicle, stained by the **Feulgen** nucleal reaction. In these photographs, the rods of the dumbbells are barely visible, not only because they are altered by the fixative, but because they are not very rich in chromatin.

These photographs include nuclei located either in the terminal meristem or in its vicinity, or in a more distant region, up to the base of the radicle. The nuclei are larger the further they are from the terminal meristem. A close study of the photographs in plate 75 leads to the following conclusions:

1. The nucleus of the radicle of *Vicia faba* is made up of one or two nucleoli held in a fixed position by a network of dumbbells with polygonal meshes;

2. The chromocenters or karyosomes are agglomerations of dumbbell balls located at the apex of the angles of the meshes; the smallest comprise 2 or 3 balls, the largest approximately 6 to 10 balls. The rods of the dumbbells that constitute these large karyosomes give them a star-like appearance (B 4, fig. 1, 2, 3, 4);

3. The number of chromocenters in the nucleus is very variable and depends on the size of the nucleus. This number is considerably higher than the number 8 to 13 indicated by **P. Dangeard.** This number is so high that it seems impossible to fix its value by counting throughout the thickness of the nucleus;

4. The number of chromocenters or karyosomes has no specific meaning, each of them being made up of several dumbbell balls in variable numbers. Only the total number of dumbbells could have a meaning.

But the considerable variations in this number according to the size of the nuclei in Figs. 1 to 10, compared with those in Figures 13, 15, 17, 18, and 19, make it certain that there is no fixed number of dumbbells in the nucleus of *Vicia faba*;

5. The existence of the dumbbell network, apparent in figures 13, 14, 16, 17, 18, 19, 28, 32, is further established by the formation of chromosome bands showing the meshes of the network (fig. 20, 22, 23, 24) in prophase and by the reappearance of these meshes in the chromosomes at the end of anaphase (fig. 21, 25);

6. The assertion by **Schæde** and **Weber** of the absence of figured elements and structure in the nucleus of *Vicia faba* therefore constitutes proof of the total destruction of these elements by the fixatives in the nuclei they examined.

* * *

In summary: For the reasons indicated above, there cannot be nuclei with a nucleolus and no nuclear network; the presence of the nucleolus necessarily implies the existence of the network that keeps it in a fixed position and that constitutes its peripheral zone by the balls of its dumbbells.

There are therefore no homogeneous types of nuclei, nor nuclei without chromocenters, nor unstructured types of nuclei.

All nuclei have a structure that, at its simplest, is made up of a single row of radiating dumbbells (euchromocentric or prochromosome type) and, at its most complicated, by a network with very many polygonal meshes formed by a large number of dumbbells whose balls are articulated either to the nuclear membrane, or to the periphery of the nucleolus, or to each other to form the chromocenters, or nodal granulations of the meshes.

Between these two types exist all the intermediate degrees as a complication of the network, but the type remains the same and unique.

Let us recall that it is exactly the same for the cytoplasmic network and that this, constituted in very young meristem cells by a single row of exactly radiating dumbbells, becomes progressively more complicated as the cell ages; it then successively comprises 2, 3, 4, then a greater number of rows of dumbbells, as it is further from the terminal meristem.

In the first part of this work it was demonstrated that the constitution of the cytoplasm of the cell is exactly the same as that of the nucleus, the space between the cell membrane and the nucleus being occupied by the network of cytoplasmic dumbbells, these being articulated exclusively by their balls either to the cell membrane, or to the external face of the nuclear membrane, or to each other to form the polygonal meshes of the network at the angles of which these balls, united in groups of 3 to 5 or 6, form masses called nodal granulations which are in short the chromocenters of the cytoplasmic network, because they differ essentially from the nuclear chromocenters only because they

do not contain or only very little true chromatin and are colored only a light pink and not a bright red by the Feulgen nucleal reaction; the cytoplasmic and nuclear networks are organized in such a way as to fulfill the same mechanical role of support and fixation with respect to the cell, the nucleus and the nucleolus; There is therefore complete unity in the form of their constituent elementary organelle, the dumbbell, in its mode of arrangement in a network, and in its mechanical role in the cytoplasmic and nuclear networks.

This morphological and functional unity is not limited to the plant kingdom; it extends so perfectly to both kingdoms that it can be difficult to distinguish at first glance in photographs whether certain cells belong to a plant or an animal.

How can we believe that there can be four different types of nuclear organization when the cellular organization appears identical in both kingdoms?

Also, in the face of such morphological unity, it is difficult to believe that it is not accompanied in both kingdoms by the unity of the processes of multiplication and growth of cellular elements and not by processes that vary even from one species to another.

CONCLUSIONS

1. Nuclei of the euchromocentric type are made up of a single row of radiating dumbbells having exactly the direction of the radius of the nucleus and whose external ball is articulated to the nuclear membrane, the central ball to the periphery of the nucleolus;

2. Nuclei of the reticulated type are made up of a network of dumbbells articulated to each other by their balls and on the other hand to the nuclear membrane and to the periphery of the nucleolus, which it maintains in a fixed position.

The chromocenters of this nuclear type are either groups of dumbbell balls, or dumbbell balls articulated to the nuclear membrane or to the periphery of the nucleolus or close to it;

3. The micronucleoli and annex bodies of **P. Dangeard** are isolated chromocenters consisting of a single dumbbell ball;

4. The satellites of the chromosomes are one or two of their constituent dumbbells not covered with matrix; the filament that unites two truncated chromosome fragments is of the same nature. The cleavage of the satellites does not exist;

5. The chromonematic filaments of the chromosomes are constituted by the dumbbells, placed end to end, of the chromosomal bands detached from the network. The balls of these dumbbells are the chromomeres;

6. The chromomeres have a real existence; they are the dumbbell balls of filaments detached from the nuclear network;

7. Due to the constitution of the nuclear network, the number of chromocenters in this nuclear type loses all meaning;

8. In the euchromocentric type, the number of peripheral chromocenters represents only half of the balls of the constituent dumbbells;

9. Between the euchromocentric nuclear type and the reticulated type with a complicated network, there must exist multiple intermediate forms that are not types;

10. The nucleolus includes at its periphery a layer of granules which are the internal balls of the nuclear dumbbells.

The existence of a nucleolus implies the existence of a nuclear network or radiating dumbbells of the euchromocentric type. There are no unorganized and optically empty nuclei.

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CHAPTER IV

SUMMARY AND CONSEQUENCES OF THE NEW CONCEPTS PRESENTED IN THIS VOLUME ON MITOCHONDRIA

AND THEIR CONFRONTATION WITH CURRENTLY ACCEPTED INACCURATE CONCEPTIONS

The historical account of the successive progress made in the study of the morphological constitution of the cell since the beginning of the last century has been given previously. I will therefore not return to it here.

I have indicated that it was especially from around 1910 that the study of mitochondria, monopolizing the attention of cytologists, led to these elements being considered distinct from the cytoplasmic network described by many authors, until that time, as a reticulum formed by fine meshes extending from the nucleus to the cell membrane.

Gradually, the study of the shapes of mitochondria led to them being considered as distinct elements, unrelated to one another.

I have previously shown that it was, for the most part, the observations of **Regaud**, **Guilliermond,** and **Lewitsky** that contributed to establishing the currently accepted notions about mitochondria, either about their shapes or about their evolution, at least in plants. The time has come to summarize the errors observed in current knowledge about mitochondria and to indicate the new knowledge that must replace it. It is currently accepted and officially taught:

1. That they have two main forms, the form of spherical granules and the form of rods or filaments of varying length (chondrioconts) and, incidentally, other forms such as clubs, filaments with several dilations in a string, etc.;

2. That they have no direct link with each other, neither with the nucleus of the cell nor with its external membrane;

3. That mitochondria are free, mobile, distinct elements, occupying a different situation in the cell depending on whether it is in a state of rest or activity, and that their very form would vary according to this state;

4. That they are capable of evolving, at least in plants, to form other elements such as chloroplasts and amyloplasts;

5. That there exist two lines of mitochondria, some active, the chondrioconts, destined to evolve into plastids, and the others inactive intended only for the reproduction of the various forms indicated above;

6. That the cytoplasm is constituted, outside the chondriome, by a system of distinct, separate cavities filled with liquid, called vacuoles and constituting the vacuome**;**

7. That mitochondria fulfill the function of catalysts;

8. That they have the property of exercising selection (**Renaut's** electosomes) among the substances they transform.

* * *

It has been demonstrated, during the presentation made so far, that all these notions are inaccurate and that this inaccuracy comes either from incomplete observations (**Altmann**), or from the altering action that the fixing reagents exert on the normal elements of the cell and from the imperfection of certain observation processes (vital observation).

It is demonstrated in this work:

1. That **Altmann's** observations were incomplete and that the figurative elements of the cell have a single form, the dumbbell, which is its constant elementary organelle and which is constituted by a rod (what has been called a chondriocont) bearing at each end a ball or granulation (designated a granular chondriosome); chondriosomes and chondrioconts therefore do not exist as independent elements;

2. That the elements of various shapes described in the cell, notably by **Gullliermond,** granules, rods, filaments, amyloplasts, are not normal elements in evolution, but only scattered debris, in the process of necrosis and disappearance, dissociated and altered by the action of the fixative and other liquids used for the preparations; that, normally, these elements are not distinct and unrelated to each other, but on the contrary intimately linked in the dumbbell organelle and in the cytoplasmic network;

3. That the dumbbell organelles are not free and mobile and that, on the contrary, they are united together to form the cytoplasmic network in which they are definitively immo bilized; that this union takes place by a constant mode of articulation, by the balls of the dumbbells, a device which explains why, as the first **Renaut** and **Gilbert** and **Jomier** pointed out, the granules of the cytoplasm are grouped in the cell *at the corners of the meshes formed by a fine network of filaments.*

This constitution of the cytoplasmic network demonstrates the inaccuracy of the notions relating to mitochondria listed above under numbers 1, 2, 6, 7;

4. That mitochondria do not evolve into chloroplasts or amyloplasts because the stages of this evolution are in reality those of their destruction by fixing liquids; chloroplasts have a completely different origin. As for the evolution of round chondriosomes into chondrioconts, it cannot exist for the simple reason that both are part of the same element, the dumbbell organelle;

5. That there cannot exist two lines of mitochondria, one active, the other inactive, because the forms indicated by **Guilliermond** as constituting these two lines do not exist in isolation in the cell, being always united in the dumbbell organelle, the only element constituting the cytoplasm;

6. That the meshes of the cytoplasmic network being open and free of any element represented like the meshes of a metal grid, the cytoplasmic space constitutes a single cavity, the cytoplasmic cavity filled by hyaloplasma and whose points all communicate freely with each other.

It has been demonstrated previously that vacuoles, cavities that would constitute the vacuome, do not exist in the normal, intact cell and that, when observed, they are the regions where the cytoplasmic network is destroyed by fixatives and by the observer's technique. *The system called the vacuome therefore does not exist* and there is only a single cavity occupying the entire volume of the cytoplasm;

7. That the cytoplasmic network represents the entire living matter of the cytoplasm and is entirely constituted by the dumbbell organelles, the debris of which are the scattered, shapeless elements, which have been wrongly considered distinct, independent, without links between them and called mitochondria.

It is therefore the totality of this living matter, immobilized in the cytoplasmic network, which is the seat of intracellular chemical transformations or which, only, assimilates the substances elaborated by the bacterial function of living beings.

If a catalytic action were exerted in the cytoplasm, it is therefore all the living matter (cytoplasmic network) which would exert it through the immobile dumbbell organelles which constitute it. But the role of ferment or catalyst cannot be fulfilled in the cell by the immobile dumbbell organelle because, in both kingdoms, the bacterial and fermentative function is exclusively fulfilled by free and mobile micrococcal granules which, in animals, are the colibacillary cocci of the blood (**Béchamp's** microzymas).

These microzymas obviously penetrate the cells and exert their action there but *do not remain there;* they leave with the liquids secreted by the cell, urine, milk, saliva,

digestive juices, etc., or with the lymph that emanates from it. *They are therefore not part of the cell, which they only pass through*. This phenomenon explains:

A) Why urine always normally contains an innumerable quantity of colibacillary cocci which are the cause of its fermentation and milk, an equal quantity of the same elements that make it coagulate and are the cause of lactic and butyric fermentation.

B) Why does saliva contain a large quantity of these colibacillary cocci, and why, in the active parotid gland of the ox, the arterial blood, upon passing through the gland, loses a large part of its fermentative power due to the loss of a large part of its colibacillary cocci that have passed into the saliva.

C) Why the active fermentative elements of digestive ferments are the colibacillary cocci from the blood that has passed through the digestive glands.

D) Why do all pathological fluids always contain the elements of the colibacillus, either in its bacillary form or in its other forms: staphylococcus or streptococcus or, when the fluids contain fibrinogen or globulins, in the form of pneumococcus or enterococcus? Because, in this case, due to their coagulant property (they are the fibrin-ferment), they surround themselves with a fibrin shell.

8. The selective action attributed by **Renaut**, then by **Regaud**, to mitochondria cannot exist since they do not exist as free, mobile elements, endowed with a fermentative or catalytic power and since this power, which the constructive halter organelles do not possess either, is exercised only by the micrococcal elements of the bacterial function of living beings, the colibacillus function of higher animals.

9. One of the main roles of the dumbbell organelles that make up the cytoplasm, a role that has remained unknown until now, is to form the cell's supporting framework. The cytoplasmic network, being formed, like a metal grid, not by alveoli, but only by polygonal meshes whose dumbbells form the sides, oriented in all directions, all connected to each other to form a single network intimately welded on the one hand to the nuclear membrane, on the other hand to the external membrane of the cell, constitutes the resistant and elastic framework of the cell, ensures the preservation of its shape, and the maintenance of the nucleus in a fixed position in the middle of the cell.

* * *

Thus, the final conclusion is that *mitochondria, distinct, free, mobile elements of varied shapes, do not exist*, at least as they have been described, and that *only the elementary dumbbell organelle, always organized in a network within the cell, exists in the cytoplasm.* These distinct, free, and mobile elements are the debris of the dumbbell organelles, and

their presence can only be observed when the cytoplasmic network is destroyed; when the latter is intact, these elements, granules, and rods are seen included in the dumbbell organelles, and these in the network.

The destruction of the dumbbells and the cytoplasmic network means that they are very sensitive to the altering action of chemical substances, fixatives, and other products, but does not mean that they are destroyed naturally and quickly after the death of the individual, as has been claimed.

This disappearance, when observed, is due to the fixative fluid and other operations performed on the tissue studied, because the persistence of the cytoplasmic network can be observed 12 hours, even 24 and 48 hours after death.

In summary, it is the inaccurate forms attributed to mitochondria by the authors who described them and the fact of having considered them as independent elements, which have led to the total ignorance of the dumbbell organelle and its essential properties which are:

1. To possess a unique form, the dumbbell form, comprising a median rod (chondriocont) bearing a ball (round chondriosome) at each of its ends. Chondrioconts and round chondriosomes therefore do not exist as distinct and independent elements;

2. To be the organelle that constructs the cytoplasmic network, which constitutes the resistant and elastic framework that ensures both:

A) The conservation of the cell's shape,

B) the maintenance of the nucleus in a fixed position at the center of the cell,

C) the free circulation, throughout the cellular space, of the cytoplasmic liquid;

3. To constitute the entire living matter of the cytoplasm, that is to say the protoplasm;

4. To be permanently immobilized in the cytoplasmic network in which, moreover, its form and evolution are stabilized;

5. To be the constructive organelle of all the tissues of the organism, notably of the entire nervous tissue. It is also that of the muscular tissue, in which it affects a very particular disposition;

6. To be the constructive organelle of all living animal and plant organisms, that is to say the universal organelle and the substratum of living matter, that is to say of life.

* * *

On page 111 of his book, *The Organization of Matter*, **Nageotte** writes: *The only criticisms that can be addressed to **Altmann** relate to his conception of the physiological value of the granula, which he considered living elementary particles, and to his views on the genesis of the cell.*

We will soon see what the life of the granula amounts to.

Further on, on page 168, he attributes to mitochondria the quality of organized ferments. But he specifies "*that he denied them the quality of living beings because life, an order of new facts, only evolves when they are assembled*."

And he adds: *They are incomplete systems, incapable of functioning alone, and they do not possess to any degree that autonomy which is one of the characteristics specific to all living beings. They are not alive, taken in isolation; it is their assembly alone that is alive.*

Elsewhere, on page 165, **Nageotte** specifies:

*The formed elements that enter the cell cannot live in isolation; the cell is therefore the elementary living unit. Its essential organelles are only fractions of units and not entire living units. As **Cuenot** excellently puts it: There is nothing living in a cell, except the whole.*

Thus, from this reasoning results the fallacy that if the organelles constituting a cell are not living, it is strictly because they are constituted by non-living, that is, inert, matter. And thus, for **Cuenot** and **Nageotte**, the cell, an elementary living unit, would be constituted by non-living, inert matter, which implies the absurd conclusion that living matter does not exist, and that life is not a property attached to matter, but only a sum of physicochemical actions whose seat would be a group of small masses of inert matter.

Now, on page 113 of his book, **Nageotte** wrote:

There is no doubt that the chondriome is always present at the moment of cell birth. There is also no doubt that each of its elements has the property of growing and multiplying by division.

Thus, an elementary organelle constituting the cell can grow and multiply, and despite these two properties, **Cuenot** and **Nageotte** deny it the quality of a living being.

However, *the property of growing and multiplying is the most essential characteristic of a living being, of life*, and the one that differentiates it from inert matter. Now, we find in **Nageotte's** book, either in the table of contents, page 554, or on page 150, this definition: *Growth by assimilation is the fundamental attribute of life.* It is therefore surprising that, while **Nageotte** recognizes in a mitochondrion the property of growing and, in

addition, the power to multiply, he denies it the quality of a living element. These are gratuitous and contradictory assertions.

On page 154, we find this explanation: *The division of growing mitochondria is explained by reasons of equilibrium between the micelles and obeys the laws of surface tension.* This is an explanation imagined to satisfy the needs of a theory; it does not have the slightest likelihood because it applies, in **Nagootte's** mind, to a mitochondrion having the shape of a granulation, a non-existent element, the elementary organelle not being this, but the dumbbell; *the very special formation of the latter proves that it is organized and not a mass of inert matter.*

The special shape of the dumbbell, a rod bearing a ball at each of its ends, necessarily imposes the notion of an organization of this element, a fact which completely changes the basis of the problem, because there can be no question of surface tension for its multiplication, as for example in the division of fat globules during of an emulsion operation.

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The entire reasoning of **Cuenot** and **Nageotte** is therefore erroneous and unfounded, because it is based on false objective data, the fundamental, real building organelle, the dumbbell, being unknown to them. The organized elementary unit is therefore not the cell: it is the dumbbell organelle, and it is this which is the elementary living unit.

Life is, in short, an internal force attached to living matter which allows it to take from the external environment the materials which suit it for its growth and multiplication, whereas a given mass of inert substance, mineral or even of organic origin, can only increase through the intervention of forces external to it, forces which determine, outside of it, the addition of new particles to the primitive mass. In the first case, living matter itself directly exerts the action; in the second case, inert matter passively undergoes it.

This is what happens to the dumbbell organelle, which draws from the internal environment, the cytoplasmic fluid, the materials necessary for maintaining its life, its growth, and its multiplication. These materials are brought to it, in animals, for example, by the blood plasma into which they have themselves been introduced by the digestion and assimilation of food, then subsequently transformed by the fermentative action of the organelle colibacillus into substances directly assimilated by the dumbbell organelle, such as fibrin, which, by depositing itself on it, thus ensures the repair of its albumin losses and, consequently, its sustainability.

The dumbbell organelle taken in isolation is alive since it multiplies, a fact proven by the creation of successive rows of dumbbells in the growing cell; its property of growth and multiplication is further proven by the fact that when, through the effect of a cause

unknown to us, it becomes Koch's bacillus, it develops ad multiplies indefinitely in its in vitro cultures and forms a true dumbbell tissue comparable, for example, to the network of dumbbells that constitutes the cells of the spinal ganglia.

The same proof is provided by the evolution of cancer, due to the disordered multiplication of the dumbbell organelle, as will be seen at the end of this volume.

These facts therefore clearly reveal the inaccuracy of **Nageotte's** assessment according to which mitochondria are not alive taken in isolation, their assembly alone being alive, and also the inaccuracy of **Cuenot's** statement: "*There is nothing living in a cell except the whole.*"

The assembly of dumbbell organelles into a network constituting the cytoplasm and, as we will see later, the nuclear network, causes them to apparently lose their autonomy; but when abnormal conditions (trauma, various destructive actions, etc.) dislocate these networks and separate these organelles from one another, their autonomy and their quality as living elements are manifested by the intense proliferation that causes and characterizes tuberculosis and cancer.

These considerations highlight the danger, for biologists, of speculations based on scientific notions considered definitive and intangible.

To support my conclusion, I will finally quote a deduction from J**. Nageotte** relating to mitochondria (22, p. 11):

The particles in question, which have received the name mitochondria, are undoubtedly highly organized. But, due to their consistency, they take on a very simple external form. For them, the organization does not have a purely morphogenic value, as for the skeletons of infusoria and connective fibers; it does not have the effect of ensuring the solidity of the living being, nor of giving rise to a resistant cohesion between the different parts, like that which exists between muscle and tendon.

Now, it appears clearly that the dumbbell shape of the elementary organelle has an essentially morphogenic value since it allows, through the articulation of this organelle with other similar ones by their balls, the creation of devices as perfect as the cytoplasmic and nuclear networks, as the neuron, a cell which, thanks to this articulation, can extend over several meters in length and include within its interior filaments of living matter of equal length, the neurofibrils, thanks to the arrangement of the organelles end to end.

The constitution of the cytoplasm, that of the myelin sheath of nerve fibers, has the effect of ensuring the solidity of the cell, that of the neuron and the nerve fiber it emits,

the maintenance of their shape and the central position of the nucleus at the same time as their elasticity and flexibility.

This is one of the essential roles played by elementary organelles, as established by the preceding studies.

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What exists in nature is therefore exactly the opposite of what **Nageotte** deduced from the erroneous knowledge concerning the form and properties of mitochondria and the cell, which he considered definitive dogmas. It is now necessary for me to discuss the value of certain attacks that occurred in 1937 against the results of my research on the nature and origin of Koch's bacillus and tuberculosis, on the one hand, and on mitochondria, on the other.

REPLY TO MR. CAULLERY'S ATTACKS AGAINST THE NATURE AND AUTOGENOUS ORIGIN OF KOCH'S BACILUS AND TUBERCULOSIS

It was during the research into the nature and origin of Koch's bacillus that I discovered that it is not a simple rod, but a dumbbell-shaped element and that it comes from elements of the same dumbbell shape that constitute the desquamated cellular elements in the pulmonary alveoli. The study of the elementary constitution of the cytoplasm and nuclei of the cells of animal and plant organisms completely confirmed these facts.

Thus, both knowledge of the universal constructing organelle of the tissues of living beings, the dumbbell, and its degeneration into Koch's bacillus were acquired to spontaneously form, without the intervention of contagion, an abnormal, pathological tissue, tuberculous tissue.

From this research, I concluded, in a general way: **that Koch's bacillus is formed by the mitochondria of the tissues of patients.**

At that time, I already knew that the dumbbell organelle is the organelle that builds the cell's cytoplasmic network and also that of the linin network of the nucleus, but I did not know whether both or just one of them, and probably the nuclear dumbbell, degenerate into tubercular bacilli. This is why I did not specify at that time and even used both terms mitochondrion and chromosome.

This term mitochondrion appeared to some to be able to serve as a basis for an attack to show that I was ignorant of the properties of these elements and that I had made a gross error in mistaking Koch's bacillus for a mitochondrion.

It must be said that at that time, the notions of mitochondria introduced by **Regaud**, **Guilliermond,** and **Parat** constituted the foundations of cytology and were respected on an equal footing with pasteurian dogmas. For example, a distinguished Parisian biologist, **E. Rabaud**, criticized in these terms (8) an unfortunate expression of one of his colleagues, **M. Caullery**, author of a book on heredity:

... *No biologist will accept that mitochondria are symbiotic bacteria or their equivalent*, a reproach to which, indignant, **M. Caullery** responded in justification:...*Now, fourteen years ago I myself energetically protested against any assimilation of mitochondria with symbiotic organisms*.

This shows the profound impregnation of minds by the errors of Pasteurian dogmas on the one hand and, on the other, by those of cytology, which, clearly, **E. Rabaud** could not have suspected. How could he have, following the whole series of concordant observations since **Altmann**?

It was precisely **Mr. Caullery** who undertook the task of reducing my conclusions to nothing. He first entrusted it to a collaborator (83), a young, incompetent and inexperienced biologist, who was answered on all points as appropriate and who, having believed he had to give a ridiculous lesson to the professors of the Museum, my colleagues, brought another upon himself by demonstrating to him, what he did not know, what gross error through ignorance consists of.

Following my response, it was **Mr. Caullery** (7) who went on the attack himself, in the Revue Générale des Sciences.

As no response was made to this attack, some readers of this magazine may have judged my silence unfavorably. This is why I deemed it necessary to make known here, first the reasons for this silence, and then my response itself.

The reasons for my silence are as follows: I had indeed written a precise, very objective, very complete response, immediately after **Mr. Caullery's** attack.

It was composed, and I corrected the proofs. But it did not appear in the Revue Générale des Sciences because influences had been at work, and my late colleague **R. Anthony**, who was its director, feared that it would harm the candidacy he had just submitted to the Academy of Sciences. I did not exercise my legal right of reply because I did not want to deprive my colleague of the slim chance he had of being part of such illustrious company and thus achieving immortality.

Allow me only to say here that the courageous attitude he had had until then in defending freedom of scientific opinion, discussion, and publication, whether by opening the columns of the Revue Générale des Sciences to me, or through personal articles, or even through a public lecture he gave at the Sorbonne, honors his memory infinitely more than his admission to the Academy of Sciences could have done.

Since my response did not appear in the Revue Générale des Sciences in 1937, it will be presented here.

* * *

The Revue Générale des Sciences of June 15, 1937, contains an article by **M. Caullery,** re lating to that of **R. Anthony** (March 15, 1937) on scientific conformism, in which he felt it necessary to affirm that my latest publication on the origin and nature of Koch's bacillus and tuberculosis deserves no discussion, and especially no publicity, and that it is a challenge to contemporary biology.

Mr. Caullery having written that, being neither a bacteriologist nor a doctor, the subject of my publication is beyond his competence, it will first appear to the reader that he would have shown good sense in refraining from formulating a judgment on a question with which he is unfamiliar. On the other hand, he wrote this article to refute the criticisms that **R. Anthony** addressed to blind conformism and he precisely justified them, as far as he was concerned, by publicly demanding, despite his lack of competence to judge them, that all discussion and all publicity be refused to the new scientific notions that I had brought forward. The criterion of blind conformism is precisely, as **R. Anthony** explained, the refusal of discussion and publicity to new notions that do not agree with dogmas.

In reality, **Mr. Caullery's** sole aim was, as he wrote, to seek to prevent any discussion and publicity about the nature and origin of Koch's bacillus, that is, to stifle this new notion which, once again, demonstrated the inaccuracy of pasteur's dogma of the asepsis of living organisms.

Here are the main points of **Mr. Caullery's** criticisms of the results of my research:

1. To argue today that the tuberculosis bacillus is not an autonomous organism, but that it is endogenous, resulting from the transformation of a mitochondrion, goes beyond the scope of bacteriology and medicine. Now, this is the basis of Mr. Tissot's entire argument. As for saying that these are facts and not interpretations, this only proves that we do not know or do not want to distinguish between the two.

This assessment, lacking clarity and precision, proved that it was **Mr. Caullery** who did not want or did not know how to differentiate between a material fact and an interpretation. The demonstration of the endogenous origin of the tuberculosis bacillus and its nature does not result from an argument; it results strictly from new positive facts, well-established and photographed material facts which are, briefly recalled:

1. The non-existence of mitochondria and the new knowledge of the dumbbell shape of the elementary organelle, the universal constructor of organized beings, and of its fundamental role in the organization of living matter;

2. The fact that the tubercle bacillus is a dumbbell-shaped element;

3. The formation of embryonic cells of tuberculous tissue by dumbbell-shaped organelles which enter into the constitution of the anatomical elements of the interalveolar partitions of the lung;

4. The formation of tuberculous tissue by embryonic cells;

5. The formation of the tuberculous bacillus and its filamentous form by embryonic cells;

6. The formation of giant cells by the embryonic cells which are their peripheral nuclei and by the dumbbell filaments which they emit and which are filaments of Koch's bacilli, a fact which explains why these cells are stuffed with the latter; this fact makes rather piquant the opinion which has been put forward that these filaments, which would be emitted by lymphatic cells, are extensions intended to phagocytose Koch's bacilli, whereas they are this bacillus itself.

To confuse these material facts with simple arguments or with simple interpretations would be to demonstrate a total lack of critical thinking. In such a discussion, a systematic denial, unsupported by any evidence, such as the one **Mr. Caullery** formulated above, only shows that it falls within the framework of the usual vague allegations of the pasteurian School against anything that undermines pasteurian dogmas.

2. "When one claims to observe mitochondria using techniques employed by Mr. Tissot and to prove their transformation into Koch's bacilli, one is committing the strict equivalent of a gross miscalculation, and everything deduced from it is rigorously worthless and deserves no discussion, and especially no publicity."

I have already responded to an identical criticism made by **Mr. Weill**, head of research in **Mr. Caullery's** laboratory. In his article of March 30, 1937, **Mr. Weill** knowingly sought to surprise the good faith of the readers of the Revue Générale des Sciences by invoking

against my research the destructive action of **Bonin** fluid on mitochondria, in such a way as to make people believe that I did not use other fixation methods, since he care- fully avoided saying, which would have nullified his criticism, that I also used a second fixation method, formalin alone, which is the most perfect for the study of the chondriome.

By formulating his criticism of the *techniques used by Mr. TISSOT* without specifying which ones, **Mr. Caullery** has therefore sought to create the same ambiguity as **Mr. Weill**, that is, to deceive the good faith of readers. I am entitled to make this assertion since **R. Anthony** has already asked **Mr. Caullery** (Rev. Gén. des Sc., June 15, 1937, p. 282, 33rd line) precisely on the subject of these same criticisms, to explain why he felt he had to ignore the response I made to **Mr. Weill**, and this request, undoubtedly very embarrassing, remained unanswered in a new note from **Mr. Caullery** (Rev. Gén. des Sciences, November 15, 1937).

I will show that, if **Mr. Caullery** did not respond, it is because he could only do so by admitting his error: I indicated, on page 25 of my book *Causes and Nature of Tuberculosis*, that I fixed the fragments of tuberculous tissue using two techniques: 10% formalin for three days, or **Bouin's** fluid.

Not a histologist will doubt for a single instant that if, in addition to the **Bouin's** fluid fixation technique, excellent for the general study of lung tissue and tuberculous tissue, I used fixation using 10% formalin alone, and especially this one, since I indicate it first, it is precisely for the observation of the chondrioma.

Now, **Mr. Caullery** wrote on page 351 (19th line) of his book *Parasitism and Symbiosis,* regarding mitochondria: *To preserve them, special fixatives such as formalin are required*. He is therefore aware that this fixation method, which is precisely the one I used, is the best; it is therefore **Mr. Caullery** who committed a gross error or used a blameworthy method by declaring my techniques for studying mitochondria defective, although he read in my text that one of them was precisely the one he himself cited as being the best.

If his criticism also targets the alleged disappearance of mitochondria immediately after the death of animals, and consequently in the fragments of tissue taken from cadavers, as **Mr. Weill** has asserted, I will respond to him, as I do to this one, that this is an erroneous assertion, contrary to the facts. I have already provided direct proof of this for myelinated nerve fibers (Rev. Gén. des Sc., April 15, 1937).

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I am able to provide new evidence of this, which are photographs of the internal structure of liver cells, cells of the adrenal capsules, and kidneys (pl. 18 to 28 bis, 3rd vol.), which show that the cell is made up of a network or framework of dumbbell organelles

in which these are united together exclusively by their balls, as I have already shown for myelinated nerve fibers, thus forming an areolar tissue.

Now, the dumbbell organelles that constitute this network or framework are also clearly visible and very well preserved in liver removed 24 to 48 hours after death (or purchased from the butcher); it remains not only intact, but still alive, long after death, as do the cells that contain them, which is not surprising since it is known, since **Claude Bernard's** experiments, that the latter continue to exercise some of their normal physiological functions long after the liver has been removed from the animal's body.

I add that I made the same observations on the mitochondria of the anatomical elements of the spinal cord, which, under certain conditions, are still intact 24 to 48 hours or more after death. In summary, if **Mr. Caullery** did not see fit to provide justifications for his criticisms, it is simply because he could not. Posing as the pope of biology who believes he can decide truth and error, he wrote that he considered it a duty to the public to make known that he considers my article of December 15, 1936, in the Revue Générale des Sciences, and consequently the results of my work on tuberculosis, to be *a challenge to all of contemporary biology*, an expression that no justification has been provided to support.

The answer I have just presented is the one that should have appeared in 1937.

Currently, I am able to provide a much more complete one. This complement to my answer is all that this third volume contains. It is above all:

1. The demonstration of the non-existence of mitochondria, as cytologists defined them, which are only the shapeless and scattered debris of cell-building organelles altered by fixatives;

2. The demonstration of the existence of a universal organelle that builds the cells and tissues of animal and plant organisms, the dumbbell organelle, an already differentiated organelle which, by articulating with others exclusively by its balls, constructs the cytoplasmic and nuclear networks in which it becomes rigorously immobile;

3. The existence of a second organelle constituting animal and plant organisms, a universal organelle of a bacterial nature, which, in higher animals, is the *Bacterium coli*; this organelle, endowed with fermentative power, is the active agent of the bacterial function, the colibacillary function of higher animals, indispensable to the exercise and preservation of life, and as important as respiration and blood circulation.

It results peremptorily from the facts presented that **Mr. Caullery** and his collaborator **Mr. Weill** have discussed mitochondria that do not exist, based on gross errors of observation and false dogmas.

I believe, this time, that I am allowed to affirm that what is a challenge to all of contemporary biology is this gross error of mitochondria, it is the gross error of the dogma of the asepsis of living organisms, which is exactly the opposite of one of the most important functions of living beings, the bacterial function; and that it is also the fact, for a biologist like **Mr. Caullery**, of combating objective, material facts, without having controlled and verified them, and this in order to defend such gross errors.

I also believe that it is also a challenge to contemporary biology to have, as **Mr. Caullery** did in his book *Parasitism and Symbiosis,* affirmed his conviction of the existence of aseptic life in animals whose organism, like that of all living beings, is constituted entirely by two organelles of a bacterial nature, the dumbbell and the bacterial granulation, colibacillary granulation in vertebrates.

This is an error that will do no more harm to the vital bacterial function of living beings than **Mr. Caullery's** anathema on the results of my research on tuberculosis and his assertion that ***they deserve no discussion, and especially no publicity,*** did to the nature and origin of Koch's bacillus.

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CHAPTER V

POLYMORPHISM OF LIVING MATTER

I. Polymorphism of the lower forms of living matter, bacteria and hyphomycetes.

II. Polymorphism of the living matter of higher fungi:

- Origin of the hyphomycetes reputed to be parasites on them;

- Origin of higher fungi in general.

III. Polymorphism of the living matter of higher plants:

- Origin of the species of Oomycetes and Basidiomycetes, Ustilaginea and Uredinaea;

- Origin of the cryptogamic formations of plants classified in the Ascomycetes, Discomycetes, Spheriaceae and Perisporiaceae;

- Nature of mycorrhizae. Their relationship with the tuberization of plants.

IV. Polymorphism of the living matter of animals.

The polymorphism of living matter results from the property of evolution that it possesses to a very high degree; it is this property that has determined the incessant evolution of living beings since the appearance of life on the globe, as well as the current form or constitution of the tissues of their various organs.

To understand this evolution, it must be considered that it is obvious that organized living matter developed at the origin of its formation on our planet in the corpuscular, that is, bacterial, form, which it has kept until our time, being constituted by two organelles of a bacterial nature: one of the micrococcal form, carrying out the chemical actions of the living being, the other, the dumbbell organelle, whose role is the construction of the framework of its tissues and having already evolved for this function.

It is therefore not the original constituent organelles that have evolved; it is exclusively the arrangement of the elements of the second to constitute the organization of tissues and cells.

The reality of this evolution of living matter is made certain by the following facts:

1. By the fact that observation of the evolution of bacterial cultures in vitro demonstrates the polymorphism of bacterial elements;

2. By the fact that the transition from the bacterial form to the mycelial form and, reciprocally, the return from the mycelial form to the bacterial form is easily provoked;

3. By the fact that the conidial forms of Hyphomycetes pass rapidly from one type to another, for example, from the *Peronospora* or *Fusarium* forms to the *Penicilium* form;

4. By the fact that the transition from the Hyphomycete form into a characterized species of Ascomycete fungi, that is, into an organized fungus of a different and much higher class, for example, the transition from the conidial form Botrytis to a characterized species of the genus Peziza;

5. By the fact that certain elements of plants continue their vegetation outside the plant in the form of *Hyphomycetes*, *Botrytis*, *Oidium*, etc.

6. By the fact that the living matter of organized beings always returns, upon losing its organization, to the original form of the elements that constituted it, that is, to the mycelial form or the bacterial form.

We will examine successively the four points of the program developed at the beginning of this chapter; this study is made and placed here because it is essential for understanding the studies that follow, particularly those relating to the variations in the biochemical and morphological characteristics of bacteria.

I. POLYMORPHISM OF THE LOWER FORMS OF LIVING MATTER: BACTERIA AND HYPHOMYCETES

We will begin this study by observing that these lower forms of living matter cannot, in general, be classified in the animal or plant kingdoms. It is completely impossible to establish this distinction for bacteria, because bacterial forms, resulting from the culture of aseptically collected parts of plants or animals, are exactly similar. For Hyphomycetes, the distinction can be established for some species but not for all. It is impossible for species of *Aspergillus* and *Mucor,* as well as for most species of *Penicilium.* On the other hand, others, such as those of the genus *Fusarium*, for example, appear to be exclusively of plant origin.

Evolutionary Cycle and Polymorphism of Bacteria

In the first volume of this work, I described (page 34) the development cycle of bacterial cultures of the bacillary form and demonstrated that this cycle includes:

1. A stage of agglomeration of the bacterial elements with the formation of spherical masses of a size varying from 2 to 15 microns or more, which I called germinative masses. The formative elements are clearly distinguishable;

2. A stage of fusion of the elements in these masses, at least of apparent fusion because, at a given moment, the mass becomes very chromatic in its entirety and the formative elements are no longer distinguishable;

3. A stage of germination of these masses with the emission of more or less long filaments;

4. A stage of segmentation of the filaments into bacterial elements of bacillary, ovoid, or micrococcal shape. Once the culture has reached this final stage, it is noted that the released bacillary elements most often continue to undergo one or more segmentations that bring them to the shape of cocci. In this case, the last segmentation shows a median line dividing the cocci into two semicircles, which will later become two perfectly round cocci.

This shows the impossibility of considering a bacterial form as characteristic of a species, since all forms can occur in the same pure culture. This impossibility results from the constant fact that any culture of a bacterium can be transformed into a hyphomycete, then the latter reduced to a bacterial form. The transition from the bacterial culture to the hyphomycete form is obtained by inoculating it onto a solid medium, meat, egg yolk, etc., or onto an uncapped, vertical agar tube. The transition takes place first in the upper part of the agar, which dries out first; the culture must be maintained at 20 to 25°.

On the contrary, to bring the hyphomycete culture back to the bacterial state, it is inoculated in a liquid medium either at a temperature of 20-25°C or at 35-37°C. If the transformation does not occur quickly, we continue with several successive subcultures; the result is also obtained by transferring to fresh broth the deposit that forms at the bottom of the culture and which generally contains bacterial elements resulting from the segmentation of the finest branches of the inoculated hyphomycete.

As an example of these facts, I recall that I succeeded, by two different processes, in transforming *Bacillus anthracis* from a very virulent culture into two different conidial forms: a *Penicilium* form and an *Aspergillus* form (see p. 522, and pl. 237 and 238 of the first volume). The *Aspergillus* spore, transferred to broth, reproduced a bacterial culture of a form exactly similar to that of the original culture. Fig. 1, plate 43 of the third volume (pl. 239 of the first volume), constitutes peremptory proof which removes any discus sion on the possibility and accuracy of these facts and which does not allow the usual banal and worthless reasons to be raised: infection by foreign germs.

I have also demonstrated the polymorphism of bacteria in a striking way by obtaining the transformation of the tetanus bacillus into another, completely different form whose properties are completely modified by the simple contact of oxygen with the tetanus toxin (see vol. 1. p. 490).

The polymorphism and instability of hyphomycete species decisively support the existence of the same properties in bacteria.

The polymorphism of hyphomycetes was established, in my first research, by a series of cultures in which I obtained the passage from one genus to another. (1st vol., p. 301). I obtained the transformation of the genus *Mucor* into *Sporothricum* and *Penicilium*, of the genus *Aspergillus* into *Sterigmatocystis*... etc. These transformations are in formal opposition to accepted dogmas, but are no less certain for that, because I was able to obtain several of these transformations on the very fact of their production and photograph them. This is, for example, the birth of a sporipherous hypha of *Penicilium* (fig. 6, 7, pl. 150, 1st vol.) on a *Conidiophore* of *Botrytis cinerea* of the vine, or the birth of a *Passalora* form on a *Monillia* form Moreover, such transformations are even less astonishing than that of Borytris, whose sclerotia are capable of giving rise, on the surface of the earth, to their normal conidial form and, if buried, to an *Ascospora* form, a *Pezize, Sclerotinia fuckeliana*, an organized Discomycete fungus. This transformation, known since the last century, is not contested.

Thus, the same living matter can therefore take on four forms:

1. The normal form of the organized tissues of the plant;

2. The bacterial form if these tissues or *Botrytis* are inoculated in broth;

3. The hyphomycete form *Botrytis cinerea*, an abnormal form of the living matter of the plant resulting from the vegetation, outside the epidermis, of certain elements of the liberoligneous bundles;

4. The form of an organized fungus, of a *Pézyze*, if we bury the sclerotia of *Botrytis* under a light layer of damp sand.

This is more than enough to demonstrate the extreme polymorphism of living matter.

* * *

I had thus indisputably established in 1926:

1. The mechanism of bacterial multiplication and the four phases of their evolutionary cycle;

2. The variability of bacterial forms;

3. The transformation of bacterial species into hyphomycetes by appropriate changes in culture conditions, a transformation that I obtained for almost all known pathogenic bacteria;

4. The transformation of the conidial forms of hyphomycetes into other conidial forms;

5. The return of the conidial forms of hyphomycetes to the bacterial form, under specific cultural conditions. These transformations, brought about by simple changes of environment, establish the same polymorphism in hyphomycetes as in bacteria.

After these results, I obtained at will the transformation of the living matter of plants and animals into bacterial forms and into hyphomycete forms. These experiments will be developed in another chapter where their significance will be explained.

* * *

In 1931, in various publications, **G. Enderlein** challenged the monomorphism of bacterial species and described a cycle of their development (17A).

In these publications, **Enderlein** devised a very specific, complicated terminology that has nothing in common with the usual terminology and makes it unintelligible to those who have not assimilated it. Here is the analysis of his main publication, entitled: *On the Rhythm of Changes in the Form of Bacteria:*

*Regarding a work by **Ludwig Schmidt-Kehl** on the different forms of two species of sarcina, the author recalls and clarifies the classification he devised to describe the various phases of the morphological evolution of bacteria. Starting from the spherical form (mychit), we pass, as the organism elongates, through the forms **dimychit**, **didimychit**, **tetradimychit**, **octodimychit**, to finally arrive at a bacterial filament or **syndimychit**.*

*Each of these phases, characterized by the presence of one of these forms, also receives a name; or rather, as there is a constant passage from one form to the neighboring one, each of the phases considered by the author is characterized both by the presence of a determined form and by the appearance of the immediately superior form. Thus the name **basit** designates the phase where we observe the spherical form associated with the first oval form; **phytit** is the following phase where the first oval form is associated with the slightly more elongated form; then come the **rhabdit** and **linit** phases after which the long chains appear (**ascit** phase), then the mycelial skeins (**mycascit** phase). But, in each phase, the author also goes to great lengths to distinguish three periods according to whether the less elongated form dominates (period designated by the prefix "**pro**") or whether it is equal to the other (prefix "**iso**"), or finally whether it is the other which domi-*

*nates (prefix "**ana**"); we thus obtain a total of 15 periods. It should be noted, moreover, that this classification leaves aside, for the sake of simplicity, the differences resulting from the internal structure of bacterial bodies and that it applies only to cases where differentiation occurs in linear form (**Katatakte Stäbchen**).*

In summary, for **G. Enderlein**, the evolution of bacteria occurs through a progressive linear elongation of the elements which, starting from the simplest and lowest form, the micrococcus, a spherical granulation, would successively take on, as it lengthens, the forms: ovoid, then short bacillary, increasingly long bacillary, then filamentary which would be the most evolved form that he calls culminating. Such an evolution obviously implies that the "bacterial thread" is formed by elements which, once formed, came to be placed end to end in a long line.

Let us first observe that this evolution does not teach us how each bacterium was formed, how it originated and, secondly, that it is exactly the opposite of the real evolution described by **G. Enderlein.**

Indeed, evolution takes place, first by the formation of a long, continuous filament, entirely basophilic, which then segments into bacilli which themselves undergo, depending on their origin, several successive segmentations through their middle, thus decreasing in length until the spherical form, which is the micrococcus and which marks the end of the evolutionary cycle.

The origin of the filament was explained in the first volume of this work; it is a mass of agglomerated bacteria, which I called the germinative mass, which gives rise to it; in germinating, each mass emits one or more filaments, exactly like a hyphomycete spore, like a basidiospore, or an ascospore. The filaments lengthen until the entire basophilic chromatic substance of the germinative mass has passed into them.

As for the latter, its mode of formation was also described and demonstrated in the first volume. Once segmented to their smallest dimension, the bacterial elements, granules and bacilli, agglomerate, forming more or less regular spherical masses, because an agglutinating achromatic substance, deposited on them, keeps them attached. When the mass has become completely chromatic, it germinates. Being made up of distinct elements that have joined and fused together, we can consider these as playing the role of male and female gamettes and assimilate the germinative mass to a zygospore formed by the copulation of two mycelial filaments.

This germinative mass, its mode of formation, and its germination escaped **Enderlein**; it is because he did not see them that he was able to believe that bacteria evolve by elongation, as bacteriologists believe, taking the short, ovoid elements of a bacillary culture for a young form destined to elongate, whereas they can only evolve by shortening

through new segmentation since they already come from a bacillus twice as long that has segmented into two parts.

Once the long filament is formed, it segments into bacilli of a certain length, which detach and continue to segment until the micrococcus form, unless the segmentation stops at the cocobacillus or short bacillus stages; they can also remain united in chains and continue to segment until the micrococcus state; the chain has then taken the streptococcus form. This often has the appearance of a chain of diplococci because the elements of each of these are the product of the last freshly completed segmentation. When the separation of the segmented elements does not take place, the filament is a chain of cocobacilli or very short or more or less long bacilli.

The evolution of hyphomycetes is exactly similar to that of bacteria; we can convince ourselves of this and find formal proof of the evolution I have just indicated by sowing *Aspergillus* or *Penicilium* spores on agar and following their evolution day by day.

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We see the spores, gathered in small clusters, emitting mycelial filaments rich in chromatin, elongating, and then finally emitting their spores, each of which is exactly the equivalent of a micrococcus.

We thus see that the basophilic matter is contained in a central canal of each filament, thus forming an uninterrupted column which goes from the germinative mass to the end of the sterygmata.

At a given moment, this column gradually decreases in length from the germinative mass because its soft, plastic substance gradually flows out of the end, these in the form of spores or conidia.

Thus, the basophilic substance constitutes in both cases, hyphomycete and bacterium, a long filament which, for the hyphomycete, flows into micrococci which are spores and segments into bacilli then micrococci for the bacterial form; the micrococci coming from a segmented bacterial filament are therefore the equivalent of the spores of a *Penicilium.*

In summary, the evolutionary cycle described by **Enderlein** for bacteria is not exact; the elements of the bacillary form do not develop by elongation; on the contrary, they evolve by decreasing in length through segmentation. With **Enderlein's** evolutionary cycle, the micrococcus is the lower form, the starting point of evolution. With the actual evolutionary cycle, well established by numerous figures in the first volume, the segmentation of the long undivided mycelial filament is the starting point of the formation of bacilli, the phenomenon that determines their initial length and brings about their

release; the continuation of segmentation on the released bacilli and also on the bacilli that remained in chains, reduces their length to the micrococcus form, which is therefore the terminal element of evolution and not its starting point as **Enderlein** indicates, while the long mycelial filament, which he indicates as the culminating form, is only the form of beginning or primitive.

The need for a germinative mass large enough to give rise to the mass of one or even several filaments that will segment into bacillary elements demonstrates the absolute necessity of this four-phase evolutionary cycle that I described in the first volume. This demonstrates that all bacterial forms indeed come from segmentation and not from elongation by growth, which exists exclusively for filaments formed by the germinative mass.

All bacillary lengths resulting from segmentation, **Enderlein's** forms mychit, dimychit, didimychit, etc., therefore do not represent phases of bacterial evolution (basit, phytit) as he indicates. These forms only represent the lengths of the segmented bacillary elements, a fact demonstrated by **Enderlein** himself, who indicates that, in a culture, the numbers of elements of each length are almost always in the ratios of 32-16-8-4-2-1.

The evolutionary cycle reaches the culminating form, according to **Enderlein**, when the element reaches its greatest length. Now, this culminating form, which is the long filament, is reached in a single step starting from the germinative mass, which remained unknown to **Enderlein**, and bacillary evolution consists exclusively of successive segmentations of this filament and its already segmented elements, then, of their agglomeration and fusion. The hyphomycete form itself can be reached in a single step by the germination of the agglomeration mass, if the environmental conditions are favorable.

We must therefore conclude that **Enderlein's** evolutionary cycle does not exist and that the cycle of the real evolution of bacteria and their multiplication is carried out by the four phases that I have outlined above, which are agglomeration of the elements into a germinative mass, fusion, germination, and segmentation of the filaments.

In reality, since it is a closed cycle that repeats itself indefinitely, there are only forms of evolution, none of which is primitive nor terminal, and all of which are equivalent; there is only one mechanism of multiplication, in which micrococci and bacilli unite to form the germinative masses (which are the leukocytes of the cultures) and these germinate to form the filaments which, by segmentation, remake bacilli and micrococci.

We now have another case to examine: that of filtering viruses, which would be made up of elements reputed to be invisible and which pass through the filters.

Filtering Viruses

The new concept of filtering viruses has been presented and is still being presented as constituting a major advance for bacteriology and an important element for future progress.

Is this assessment justified?

It is not, for the following reasons, which I consider peremptory: 1. It is the viruses of eruptive fevers, smallpox, vaccinia, chickenpox, sweating sickness, scarlet fever, measles, foot-and-mouth disease, etc., which led to the admission of the existence of so-called filte ring viruses because the virulent liquids of these diseases did not lose their virulence after passing through the wall of a very fine filter.

But the main and real reason for this name is that it has never been possible to find, in virulent liquids, figurative elements, especially microbes, which are the active agents; it has been falsely concluded that, if these agents have not been seen, it is because their size is less than the limit of visibility at the highest magnification of the microscope.

This conclusion is false because, instead of being so small, the active elements of eruptive fever viruses are, on the contrary, very large (microscopically speaking), their main reproductive elements, the conidia, being about 4 to 6 microns wide, and 5 to 8 or more long for smallpox, vaccinia, and chickenpox; for measles and scarlet fever, some spherical virus elements are up to 12 microns and more (see first volume: Measles p. 556 and p. 250; Scarlet Fever, p. 564 and pl. 256 fig. 1, 2, 4, 5, 6).

Many observers certainly had these large elements of viruses before their eyes, but did not see them because, for a bacteriologist, the active agent of a virus is above all a microbe. Only **Cohn** (*Microsphæra vaccinia*) and **Guarneri** saw globular bodies in the vaccine, without, however, proving their specificity, bodies seen again by **G. Enderlein.**

The virulent agent of smallpox and vaccinia is a hyphomycete that completely destroys the dermis with its mycelium at the level of the pustules. In Plates 71 and 72, we will see the incontestable proof of the specificity of this hyphomycete for vaccinia and in Pl. 70 for smallpox. The pustules are literally stuffed with this mycelium and its conidia. It is these and the granules they contain (analogous to **Turpin's** globulins) that confer its activity to the **Jennerian** vaccine and that reproduce the vaccinal pustule.

By examining the figures in plates 71 and 72, showing sections of vaccinal pustules, the reader will wonder how anyone could have avoided seeing the virus and will certainly understand the harmful influence of the obligate microbe that the pustule does not contain.

In the entire group of diseases called eruptive fevers, the virus is therefore not invisible; if it has not been seen, it is because it is very large and is not a microbe, but a hyphomycete;

2. From the term filtering virus, it follows that the diphtheria toxin, which is the filtrate of a culture of diphtheria bacillus, is the filtering virus of this culture. It is taught that toxins are the soluble products produced by bacteria and they are distinguished, without providing any direct proof, into endotoxins produced by the internal substance of the bacteria and exotoxins produced by action on the substances that constitute the culture medium. This definition of toxins is inaccurate, the active agent of the diphtheria toxin being constituted by granules of 0.2 to 0.7 microns which, upon contact with atmospheric air, completely reproduce the original diphtheria culture in 2 to 4 days. Indeed: On diphtheria and tetanus toxins from the Pasteur Institute, I made the following observations in 1925 (see p. 187, 1st vol.):

1. Under the microscope, they are seen to consist of fine granules of 1 to 5 tenths of a micron in diameter and even more;

2. These granules pass through the filters;

3. If we bring air into contact with the toxin by breaking the tip of the sealed tube containing it, we see after 24, then 48 hours, that the surface of the liquid becomes cloudy over several millimeters in height, then that on the third or fourth day this region has become a culture of bacilli. To carry out this experiment impeccably, the procedure is as follows:

The tapered end of the sealed tube containing the toxin is heated to sterilize it, and we cap it, while it's still hot, with the long carded cotton cap of a culture tube sterilized in a flambé oven, which we then tighten tightly with sterilized wire, then with forceps, we break the tip of the tube.

This means that filtered air enters. It is also easy to check that it is indeed a diphtheria culture that has been reconstituted in the case of diphtheria toxin.

As for the tetanus toxin, it changes shape upon contact with oxygen and reproduces a complete culture of a bacillus in uniform rods 0.5 to 0.8 microns wide and 3 or 4 microns long, which is the colibacillus. The hairpin bacillus is therefore an anaerobic form of the previous one and an example of the polymorphism of living matter.

This experiment therefore proves:

1. That the diphtheria toxin is not, as is taught, a solution of toxic products of microbial origin, but a liquid holding in suspension fine granules of living substance which are constituent elements of the diphtheria culture and which are in a state of perfect emulsion because they are animated by Brownian motion;

2. That the filtering and living granules of the culture are capable, as well as the large elements, of reproducing and multiplying the bacillary form.

How can these granules reconstitute the bacillary elements? It is because, as in a complete culture, they have the property of agglomerating and fusing with each other; thus, larger granules are first formed, then germinative masses which will give rise to the mycelial filaments and, by segmentation of these, to the bacillary forms.

I will ask here a question about a fact that surprises me: since the need to preserve toxins away from air and in sealed tubes has been recognized, it must therefore be realized that, if they are left in contact with the air, they reproduce the bacillary elements, that is, the complete culture. Why then do we still maintain the definition of toxin, as it is taught, since it has been realized that it is false?

Cultures of all viruses have the same constitution; they include large non-filtering elements and granular filtering elements; these, which are found in the filtrate, very easily reproduce the larger elements.

The expressions toxin and filtering virus are therefore inappropriate, because the granular and fine figurative elements that constitute them have the same constitution as the large elements of the complete culture and are capable of reproducing them.

Let us note, on the other hand, that these granular elements of the toxins **are their only active elements** and that by removing them by adding gelatinous tricalcium phosphate to the liquid, followed by centrifugation, this treatment being repeated four or five times or even more, the activity of the toxin is suppressed.

Many researchers believe that they obtain, with the filtering part of the viruses, results that are not given by complete cultures. They will not obtain, with the filtering part, a result different from that given by the complete culture, since the two parts of the virus, filtering and non-filtering, both reproduce the complete culture very quickly.

Conclusions

1. In reality, the term filtering virus has no meaning; the filtering part reproduces the complete culture with its large elements very quickly;

2. It is incorrect that reputed filtering viruses do not contain visible figurative elements; their active elements are visible;

3. The main viruses that gave rise to the name filtering viruses, those of eruptive fevers, are hyphomycetes, some of whose elements are 3 to 12 microns wide or in diameter (smallpox, vaccinia, chickenpox, measles, scarlet fever);

4. The toxins, filtered viruses, therefore filtering viruses, are only active through the granular elements of 0.1 to 0.8 microns that they contain; these elements remain inert only if they are completely deprived of oxygen;

on contact with atmospheric air they evolve on the contrary rapidly and reproduce, in 24 to 48 hours, the complete original culture. The name filtering virus is therefore not suitable for toxins because it has no meaning; the name toxins is not suitable for them either, because their active elements are not toxic elements, but figurative, granular, living elements of the culture from which they come, and possessing, like all living granular elements of 0.1 to 1 micron capable of reproducing, the fermentative power.

Conclusions regarding polymorphism in bacteria

With regard to the specific characteristics of bacteria and the fixity of the species, in general, it appears:

1. That every bacterial species is only a temporary form of living matter originating from an organized being, animal or vegetable;

2. That monomorphism does not exist among bacteria and that, under the influence of modifications of the various environmental conditions, every bacterial species can undergo the most diverse modifications of form and even leave the bacterial state to take the hyphomycete form, a fact that I have formally demonstrated for almost all known pathogenic microbial species (1st vol.);

3. That, consequently, any change in form or character of a bacterial species can only be considered temporary and susceptible to being modified by variations in environmental conditions;

4. That a bacterial race, obtained by modification of an original virulent source by means of a change of culture medium, is liable to return to the primitive type and to regain its virulence if it is transported again into a third culture medium, the animal organism for example. It has also been known for a long time how easily one can, by a simple change of medium, exalt or suppress the virulence of a microbial species, or modify certain of its properties;

5. That the form of viruses is not only the bacterial form but also the hyphomycete form, the latter being generally the primitive infective form which gives rise, in the infected organism, to the **secondary** bacterial form.

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II. POLYMORPHISM OF THE LIVING MATTER OF HIGHER FUNGI

Higher fungi are often covered with a mold reputed to live parasitically on them. According to accepted doctrines, the spores or conidia of this hyphomycete have an external and heterogeneous origin and are transported by the wind, insects, or other animals. Now, it is known that, for most of these hyphomycetes growing on fungi, their exclusive habitat is a well-defined species and is not found on other species; this is a well-established specificity that we find in the Uredinaceae and the Spheriaceae; for example:

Harziella capitata grows exclusively on *Tricholoma nudum*,
Didimopsis helvelle grows exclusively on *Helvella lacunosa,*
Diplocadium minus grows exclusively on *Polyporus versicolor,*
Micogon puccinioides grows exclusively on *Russula rubra.*

The spores of these species, carried to other mushrooms, to damp earth, or to sterile, damp blotting paper, develop there. Now, if insects or the wind carry them, they spread them everywhere, and the same mold should be found on the earth, on dead leaves, on other mushrooms where they are never observed.

Often, the mold of the mushroom develops in such a way that it extends over the ground 10 centimeters from the foot. How, under these conditions, can we believe in the transport of spores by the wind or insects? The cause of the development of mold is obviously different, and its exclusive and specific habitat is obviously due to a specific cause. This specific cause is that mold originates either from the germination of the spores of the fungus of which it is a conidial form, or from the living matter of the foot or cap, deteriorated by insects or linaces and partly putrefied, or even from the total mass of the putrefying fungus.

This origin is all the more evident since the spores of all species of fungi necessarily germinate to form the mycelium that gives rise to them; this mycelium takes on conidial forms that are known for many species.

This will also be found and demonstrated later on with *Nectria solani*. The specific link that exists between a fungus and the mold that grows on.

There is yet another specific link that is related to the origin of the fungus. We saw above the specific relationship that exists between *Botrytis cinerea,* an emanation of the living matter of the vine, and certain ascomycete fungi, the Peziza fungi of the genera *Sclerotinia* and *Stromatinia*. Here, the specific link is formal between the plant, the Hyphomycete emanating from it, and the Peziza growing on the sclerotia produced by the hyphomycete.

An identical specific link is made between the potato, *Spicaria solani*, a hyphomycete formed by its living matter, and *Nectria solani*. Here, the link is even more formal because *Nectria solani* grows directly on the potato; The specific link is demonstrated by the fact that the spores of *Nectria solani* give rise to *Spicaria solani,* which is its conidial form (**Reinke**), and that the normal parenchyma of the center of a potato, removed aseptically and cultivated in vitro, also gives rise to this *Spicaria solani.* For many fungi, there is a specific link with a specific plant. This link is direct when the fungus grows directly on its stem, indirect when it grows only on the region of the soil occupied by its roots; the direct link exists for the genera *Pleurotus, Lentinus, Panus, Trametes, Polyporus*, etc.; the link is indirect for many species indicated as growing in the ground under certain specific tree species, such as, for example, certain truffles exclusively under oaks (*Tuber brumale* and *Tuber melanosporum*).

In the first category, *Pleurotus eryngii,* which grows at the base of the stem of *Eryngium campestre, Pleurotus acerinus* on maple trunks, *Pleurotus pometi* on apple tree trunks, *Pleurotus moricola* on mulberry tree trunks, etc., have such a precise specific relationship with the plant that it is impossible to admit that the fungus results solely from the parasitic penetration of the mycelium developed by the spores. The fungus must be an external emanation of the liberoligneous bundles of the plant, vegetating in a modified form. As with *Nectria solani*, one of the conidial forms of the living matter of *Eryngium campestre* and those of *Pleurotus eryngii* will show, by comparison, whether one of these conidial forms is common to both.

If the specific relationships between higher fungi and certain plant species have not attracted attention, it is because the monomorphism of living matter prohibits the thought that an *Eryngium*, for example, could give rise to a *Pleurotus*. However, the fact has been formally demonstrated for *Sclerotinia fuckeliana*, emanating from the vine, and *Stromatinia fructigina* emanating from a plum. We will see other cases of the same kind.

Franck (1885), **Schlicht** (1888), and **Janse** had concluded from their research that the roots of a large number of woody and herbaceous plants are parasitized by various species of *Fusarium.*

We have seen elsewhere that **Galippe** (23) (1887) also concluded from his research that many plants, carrots, turnips, cabbage, celery, etc., are parasitized by microorganisms and that, consequently, they appear to offer only very little resistance to the penetration of bacterial genera.

Noël Bernard (5 bis), placing well-washed potatoes in a humid chamber, almost constantly obtained the development of *Fusarium solani* on their surface. In other experiments, he noted that: ''*Fragments of tuber parenchyma, removed aseptically and placed in a humid environment in sterile tubes, do not* ***generally*** *give rise to any development of Fusarium*.''

This term "in general" placed at the end of this sentence means that *Fusarium* sometimes develops on their surface. Nevertheless, **Noël Bernard** then adds: ''*Now, if we sow Fusarium solani spores on these living and sterile fragments of tubers, this fungus develops there, although poorly*.''

And he concludes: "*That Fusarium therefore does not normally exist in the parenchyma of the tubers, which appears to be an aseptic environment*."

Finally, **Noël Bernard** concludes from these facts:

1. That there is an exact coincidence between the infection and the tuberization which is its consequence and a symptom;

2. That *Fusarium* always parasitizes the roots, but does not exist in the other parts of the plant, stem, leaves, and fruits;

3. That parasitism is proven by the fact that plants born from potato seeds or fruits do not form tubers because they do not contain *Fusarium.*

The various conclusions stated above are based on inaccurate facts; in fact:

1. The central part of the parenchyma of tubers removed aseptically in the form of pulp and cultivated on agar gives rise to *Fusarium solani* (pp. 326, 327, 1st vol.);

2. The formation of *Fusarium solani* on the pulp thus cultivated has no connection:

A) Nor with an infection of the tubers, because the most attentive microscopic observation of the parenchyma of the tubers never detects the presence of the fruiting bodies so characteristic of *Fusarium.*

B) Nor with the formation of tubers or tuberization, because the pulp of their parenchyma, cultivated on agar, gives rise not only to *Fusarium solani,* but to other conidial forms specific to the potato: *Spicaria solani, Sporothricum, Citromyces, Penicilium*, and even *Phytophtora infestans*, which could just as well be considered as agents of tuberization and as infective agents;

3. The stem and fruits, cultivated aseptically (p. 327, 1st vol.) also give rise to *Fusarium solani* and *Spicaria solani*, as well as to the other conidial forms of Hyphomycetes that I have just mentioned. If, therefore, the sown fruits and seeds produce plants that do not produce tubers, this demonstrates that tuberculization has no connection with *Fusarium solani* and the other conidial forms of the potato;

4. The cause of potato tuberization by root infection is based not only on errors of material facts, but also on an error of reasoning: in fact, in all parts of the globe, the potato tubers and plants that exist by the billions in almost all countries all contain, without exception, the elements capable of giving rise to *Fusarium solani* and other conidial forms of the plant.

Now, one is only authorized to conclude that representatives of a species of living beings are infected if there are unaffected, uninfected individuals. Among billions of potato tubers, there is not a single one that does not contain the elements producing the conidial forms of the plant, and there cannot be, because it is inherent in the nature of its living matter. This fact alone demonstrates that there can be no question, in this case, of either parasitism or of symbiosis.

The facts on which **Noël Bernard** relied to conclude that the cause of potato tuberization is an infection of the roots and tubers by *Fusarium solani* are therefore erroneous. Only the in vitro production of *Fusarium solani* (as well as other forms of hyphomycetes) by all regions of the tuber is accurate, a property also exhibited by the stem and fruits and which has no connection with tuberization, as shown by the previous research of **Franck**

(1885), **Schlicht**, (1888), **Janse**, who had concluded that the roots of a large number of woody and herbaceous plants which, however, do not form tubers, are nevertheless parasitized by various species of *Fusarium*.

However, one qualification must be made regarding the conclusions of these various authors. They concluded that these endophytic fungi do indeed exist in the normal, healthy parenchyma of the plant. This is not correct; they do not exist there.

The fact that, by culturing the parenchyma of a plant, a form of hyphomycete is obtained does not prove that this form itself exists in the normal parenchyma; this is the case, for example, with a bacterial culture whose elements are transformed into a type of *Aspergillus* that the original culture did not contain; but the fact that the parenchyma of a plant gives rise, in an absolutely constant way, to a characteristic conidial form that only it can give, proves that there is a specific link between the hyphomycete and the living matter of the plant, that is to say, that it is a special morphological state of the latter and one of the forms of transformation of its living matter.

The living matter of the plant affects, in its organization, both the mycelial form and the bacterial form, but the mycelial form does not appear in the usual conidial forms of the hyphomycetes with the characteristic fructifications.

One would search in vain, in the parenchyma of a potato tuber, for the characteristic fruiting body of *Fusarium*, whose shape is identical to that of a pea pod; in this potato parenchyma, scraped and placed on a slide, one can easily observe the large mycelial trunks that ramify into multiple, fine branches. This is therefore a special mycelial form of the living matter of the potato, and it is obvious that it is this mycelial form which, removed from its natural environment, takes on a different form, the *Fusarium* form and the other conidia forms mentioned above.

In this scraped parenchyma, one also sees numerous micrococcal granulations of various sizes, very basophilic, which, the polymorphism of living matter being established, explain why they can give rise to bacterial cultures, as **Galippe** obtained them.

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The following fact will support the conclusions just drawn about the potato: another formation develops, an *Ascomycete* fungus, *Nectria solani*. From the spores of this *Nectria*, **Reinke** obtained the conidial form *Spicaria solani*, which, as we have seen above, is also one of the normal conidial forms of the healthy and normal living matter of the potato parenchyma. It follows, therefore, that *Nectria solani*, an *Ascomycete* fungus, is an evolutionary form of the normal, organized living matter of the potato.

In the first volume of this work, *Nectria ditissima*, which is believed to be the cause of fruit tree canker, has already been studied, as has *Nectria cinnabarina*; on the mycelium of *Nectria ditissima*, pluriseptate conidia are born, which are those of *Fusarium vilkommii,* and then perithecia, whose asci contain 8 ascospores. For *Nectria cinnabarina*,

Tubercularia vulgaris is the conidial form of the mycelium on which the red-orange perithecia develop.

In both cases, the mycelium that begins the lesion is not the initial cause, but the result of the degeneration of the anatomical elements affected, either by a cultural cause, an injury caused by insects, or frost. The drying out of a branch has already begun, its decline very visible, when the perithecia of *Nectria cinnabarina* appear.

For all lesions of higher plants called cryptogamic diseases and characterized by the growth of bacteria or *Oomycetes, Basidiomycetes,* or *Ascomycetes*, there is a certain specific link between these and the affected plant species. Thus, to give a few examples:

Ustilago maydis is specific to maize,
Ustilago panici-miliacei is specific to millet,
Ustilago tritici of wheat is incapable of infecting other cereals,

Puccinia dispersa is specific to rye,
Puccinia tritricina is specific to wheat,
Puccinia agropyrina is specific to Agropyrum repens,
Puccinia triseti is specific to yellowish oats.

We could continue this list by adding most of the Ascospore fungi and many of the Hymenomycete fungi, as I have indicated, for example, for *Pleurotus eryngii*, *mori*, etc.

It must be concluded from this statement that the fact of a specific relationship between the plant bearing the lesion and the bacterium or Hyphomycete reputed to be the infecting agent constitutes proof that these are an abnormal vegetative form of the living matter of the carrier, that is, that the lesion or disease is autogenous.

It is the same in animals. A certain specific link had long been observed between Koch's bacillus and the species in which it develops—humans, cattle, birds—a specific link corresponding to very clear differences in properties, but no particular importance had been attached to it and the necessary conclusion had not been drawn. I had already drawn this conclusion in the first volume of this book in 1926.

By a precise determinism of the evolution of tuberculous lesions in the human lung, I demonstrated in the second volume of this work, in 1936, that *Koch's bacillus* is the dumbbell organelle, an elementary organelle, builder of the organism's cells which, through degeneration, has acquired a faculty of multiplication which constitutes its tubercular power, a fact which led to the knowledge of the spontaneous development of tuberculosis.

Thus, the nature of the link between Koch's bacillus and humans, cattle, or birds was materially established, a link which signifies that there is in reality between human, bovine, or avian bacilli the same specific difference as there is between a man, an ox, and a hen.

The same fact is reproduced for the colibacillus. Until now, it was believed to be a heterogeneous microorganism, and it has been observed that there are numerous races differing from one another in certain of their properties. Now, it is demonstrated, in this third volume, that the colibacillus is an essential, capital constituent of the animal organism and the active agent of its colibacillary function, that is, one of the constituent elements of its living matter. This fact explains why the colibacilli of humans, cattle, horses, etc., differ specifically from one another and have properties that vary for each species.

As with human, bovine, and avian Koch bacilli, the differences between the races of colibacilli were well recognized, but they did not attract attention because the harmful pasteurian dogmas of atmospheric panspermia and the asepsis of living organisms forbade the idea that there could be a direct, specific relationship between bacterial elements and living organisms. These dogmas are the absolute negation of the mycobacterial nature of animal and plant organisms.

For plants, the specific link between the plant and the bacterial or mycelial agent, reputed to be infectious and heterogeneous, could not be perceived any more for the same reason.

And this prohibition, decreed by pasteurian dogmas, has forced, for almost three-quarters of a century, to imagine erroneous explanations in order to bring all the new facts into line with them, which further distorted the direction of science.

In almost all questions of bacteriology, one could find hundreds of errors of this kind, and false conclusions resulting from the observation of dogmas. A certain number are noted in the chapter: *Errors in Bacteriology.* This fully justifies the opinion I have expressed elsewhere: **current dogmatic bacteriology is a deception**.

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It is superabundantly proven by the often so complicated metamorphoses of marine animals and by those of insects. It is also proven by the multiplicity of forms of tissue organization.

In the same organism, that of a mammal for example, it is proven by the simultaneous existence of two organelles: the fermentative organelle, colibacillary granulation fulfilling the general bacterial function of living beings and the universal constructor organelle.

This polymorphism is proven by the various forms that the colibacillary granulation takes when developing into bacilli, coccobacilli, streptococci, etc. It is further proven by the undeniable fact, easy to reproduce, of the formation of a fragment of tissue, cultivated in vitro. Formal proof of the reality of this transformation is given by the fact that part of the white layer surmounting the centrifuged blood cells of horse or ox blood inoculated in broth gives rise to the colibacillus and that this, transferred to solid media, gives rise to one or more hyphomycetes.

Alongside polymorphism, we observe with each change of form a change in certain properties of the organelle considered; this is, for example, for the colibacillus, the frequent loss of the power to make indole; it is, for the normal dumbbell organelle, the development of the cancer-causing power or the tubercular power which makes it a Koch's bacillus, a power which **Calmette** and **Guérin** succeeded in making the latter lose, by culture on bile medium, which brings it back, at least in appearance, to the state of a normal dumbbell.

General Conclusions

1. One of the principal properties of living matter is polymorphism, which allows it to take on the most varied forms under the influence of the conditions of the environment in which it lives;

2. In bacteria, polymorphism transforms a micrococcus into a bacillus, coli-bacillus, streptococcus, or mycelial filament, then into a hyphomycete, then into an organized fungus, a *pézize*;

3. In hyphomycetes, polymorphism can bring them back to the bacterial state, or make them pass from one conidial form to another, from the *Peronospora* or *Mucor* form, for example, to the *Penicilium* form, or again, through the formation of perithecia, make them pass to the state of *Ascomycete* fungi (*Pezize, Sclerotinia, Stromatinia*), that is to

say to a higher class; polymorphism thus achieves the passage of the organized living matter of the plant, either immediately to the bacterial state, or to the hyphomycete state, then to the state of *Ascomycete* fungi;

4. Cryptogamic diseases of plants are not due to an infection by heterogeneous spores coming from outside; their development is autogenous and they are all due to an abnormal degeneration or transformation of their living matter.

The mycelial stroma that exists at the base of all pathological formations cataloged among the *Basidiomycetes* and *Ascomycetes* is a product of the anatomical elements of the affected plant, notably its liberoligneous bundles;

5. The living matter of animals and plants, collected aseptically and cultivated in broth and on solid media, gives rise either to a bacterial culture or to hyphomycetes of various conidial forms.

This fact does not mean that these hyphomycetes exist in these conidial forms in the organisms of the living beings from which they emanate; proof is provided that they can be developed by a bacterial culture emanating from their organism if it is transferred to solid culture media; their formation is a result of the polymorphism of the living matter;

6. The fact of having obtained hyphomycetes by culturing plant parenchyma, particularly roots (mycorrhizae), does not therefore mean that they are parasitized; the conidial forms obtained do not actually exist in the plant tissue; they arise in in vitro cultures due to the mycobacterial nature of plant tissues and through the effect of polymorphism. Mycorrhizae therefore do not exist and neither does parasitism.

The same is true for the potato; the multiple conidial forms that we obtained by cultivation of its parenchyma result both from its mycelial nature and from the polymorphism of living matter, but not from an infection, because they do not exist in their conidial forms in the tuber. All parts of the plant give rise to these same conidial forms, and not just the tuber; the central part of the tubers contains them as well as the cortical region; the facts on which **Noël Bernard** relied to justify his conclusion are inaccurate.

There is therefore no relationship between the infection of the roots and their tuberization; this infection does not exist;

7. The inaccuracy of the conclusions relating to an alleged infection of plants by *Oomycete, Basidiomycete, Ascomycete*, or *Hyphomycete* fungi results from ignorance of the mycobacterial nature and the real anatomy of the plant organism, as well as from a lack of understanding of the extreme polymorphism of living matter.

CHAPTER VI

CRITICAL STUDY OF PASTORIAN DOGMAS

THEIR HARMFUL ACTION ON THE PROGRESS OF SCIENCE

We will study successively:

I. The dogma of atmospheric panspermia.

II. The dogma of the asepsis of living beings.

III. The dogma of bacterial monomorphism.

IV. The dogma of contagion.

V. The errors of bacteriology.

For about seventy-five years, every new biological notion has had to pass through the sieve of **Pasteur's** four principles or dogmas in order to be accepted, the first two of which, the dogma of atmospheric panspermia and the dogma of the asepsis of living organisms, were peremptorily demonstrated to be false by **Béchamp** in his discussions with **Pasteur** on the subject of the putrefaction of muscles, blood, milk, and urine.

This is the origin of the struggle that the pasteurian school continues to this day to keep these false dogmas intact and prevent the truth from appearing.

Few new notions have appeared since the establishment of these dogmas because, by their substance contrary to the truth, they have kept researchers in error and led their research down false paths, thus sterilizing bacteriology and medicine for seventy-five years.

Since **Béchamp**, no one has systematically checked the accuracy of pasteur's dogmas, obviously because of the notoriety **Pasteur** acquired.

The fact that a scientist has acquired worldwide fame through a series of scientific works whose results are generally accepted does not exempt them from scrutiny and criticism. Everyone is susceptible to error, and truth does not always accompany notoriety. When an error seriously harms the general interest, the duty of the one who discovers it is to make it known, regardless of the notoriety of its author.

It must be recognized that it is unacceptable to want to impose false dogmas on science that are not supported by any certain, well-demonstrated fact and that, on the contrary, a series of exact material facts demonstrate to be false.

The facts published in the first volume of this work demonstrated, in a peremptory manner, the falsity of pasteurian dogmas; they already established the bacterial nature of living beings, animals and plants, and the fact that they are the original source of viruses of heterogeneous infectious diseases of humans and animals.

The publication of this first volume was greeted with fierce hostility, and attempts were made to suppress the new knowledge it brought, with the sole aim of safeguarding the false pasteurian dogmas and also, it must be said, considerable material interests.

My publications were banned in all learned societies, and an order was issued in most medical journals to prevent any publication on the subject.

At that time, I did not react against these procedures and I believed it more useful to make new, persevering, tireless efforts to respond to these unacceptable maneuvers with new, peremptory demonstrations, of a nature to make the protection and preservation of these false dogmas impossible.

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I already succeeded in this in 1936, by publishing the second volume of this work, and by demonstrating the autogenous origin and nature of Koch's bacillus, its nature as an elementary tissue-building organelle, and the spontaneous development of tuberculosis, facts of capital importance.

With this third volume, the demonstration of the original source of colibacille, streptococcus, pneumococcus, staphylococcus, enterococcus, etc., which are one and the same bacterial element, a demonstration which leads to the new and capital knowledge of the bacterial function, as important as respiration and blood circulation, this time completes the annihilation of pasteurian dogmas.

This function, by its capital importance, can no longer be ignored; it definitively establishes the bacterial nature of living beings; it is the exact opposite of the basis of pasteur's dogmas. A critical examination of these has therefore become essential. It is to this criticism that we will engage. We will successively study each of the four dogmas listed at the beginning of this chapter.

1. THE FALSE DOGMA OF ATMOSPHERIC PANSPERMIA

Pasteur's main experiment, already carried out and known well before him and which established the dogma of atmospheric panspermia, proves only that, in a culture medium sterilized by heat, there can be no spontaneous development of microorganisms and that, if this development takes place, it can only be due to new living germs, introduced into the liquid and coming from the external environment, notably from atmospheric air.

Fremy devoted his entire book on the generation of ferments (22) to the refutation of the dogma of atmospheric panspermia. He successively attacked the various conclusions of **Pasteur's** publications, or the assertions that he made in the lively discussions that took place on this subject at the Academy of Sciences, to demonstrate their inaccuracy; here are some of the assertions that he attacked:

1. All true ferments derive from germs born from parents similar to them: the air holds these germs of ferments in suspension, constantly sowing them in fermentable environments. All true ferments therefore come from the outside;

2. Grape juice taken from the interior of the fruit and blood removed directly from the circulation are preserved without alteration if they are protected from the influence of atmospheric dust.

3. In **Gay-Lussac's** experiment, if the air, by entering a bell jar placed on mercury containing crushed grape berries, causes the vegetable juice to ferment, it is because this air brings germs that it contains or it causes alcoholic ferment germs that exist on the surface of the mercury to arrive in the fruit juice;

4. I do not believe a fact admitted by **Mitcherlich, Cagniard-Latour**, and **Turpin**, namely that yeast globules often burst and spill their granular contents, which release seminules into the liquid, which enlarge and become ordinary yeast globules;

5. The germ of the alcoholic ferment is the germ of mycoderma vini, which is found abundantly in the air. The germ of mycoderma vini is one of the most widespread germs in the atmosphere;

6. Grape juice, the juices of all fruits, beer must, milk, blood, urine, ferment when exposed to the air, because these organic liquids find in the air and receive from it the different species of germs of the ferments that generate all the fermentations that these environments can produce.

Regarding the fourth of these affirmations of **Pasteur**, **Frémy** writes in his book:

*We can also understand the interest that **Mr. Pasteur** may have in denying the generation of yeast by the development of seminules emerging from the interior of a grain of brewer's yeast: such a fact is in opposition to the theory of atmospheric germs, and especially to that of the **first-jump generation** of yeast grains that **Mr. Pasteur** put forward to explain intracellular fermentation. **Turpin** claims that this effusion of yeast seminules occurred before his eyes. The existence of seminules inside yeast grains is certain; it can be observed under a microscope and these seminules are found in the liquor obtained by cold-grinding yeast in water... It is these organisms that produce the alcoholic ferment, and not the atmospheric germs that no one has seen.*

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It has become beyond dispute that the research of **Cagniard-Latour, Trecul,** and **Turpin** definitively established, through the example of brewer's yeast, the origin and intraorganic development of ferments, and this notion, surely established as early as 1835, destroys all the arguments that **Pasteur** subsequently formulated to establish their atmospheric origin.

On the other hand, a certain number of facts included in this argument are not correct: in particular, it is not true that blood, milk, and urine, collected and kept away from airborne germs, are preserved without putrefying; on the contrary, they ferment and putrefy.

In summary, **Pasteur's** atmospheric panspermia, understood with its principles and the applications he made of them, is a false dogma; the denial of the intracellular or autogenous origin of ferments in animals and plants that this dogma contains can no longer be supported today. The dogma is outdated.

It is true that there are living germs suspended in the atmospheric air and in the dust that settles on all objects. But it is incorrect that they have all the importance that **Pasteur** attributed to them, that they are a significant cause of contamination and, in particular, the source of the bacterial species observed in the oral, nasal, and pharyngeal cavities and in the digestive tract.

This is one of the biggest errors in bacteriology; the original source of these germs being demonstrated to be autogenous for the most part in this book, including that of the intestinal colibacilli.

This dogma is false because, by supporting the second false dogma of the asepsis of living beings, it has served to attribute the cause of autogenous diseases to external germs, notably those caused by the colibacillus and its various vegetative forms, staphylococcus, streptococcus, pneumococcus, etc., all forms demonstrated in this book as dependent on the colibacillary function of animal organisms.

Although **Pasteur** used this dogma, as will be seen later, to deny the spontaneous generation of living matter, it actually has no bearing on this question. **Pasteur's** experiments relating to it prove only that a culture broth whose living germs have been killed by heat remains capable of supporting the development of other living germs introduced into it, and nothing more.

The fact that heat destroys germ life also proves that it renders a culture medium unsuitable for spontaneous generation by altering the substances that could bring it about.

The main use of this false dogma has been, until now, to oppose it to all the new, accurate notions demonstrating the inaccuracy of the dogma of asepsis in living beings, for example when it came to verifying the results of **Portier's** experiments relating to "symbionts." The Commission of the Biological Society responsible for this task, which included Messrs. **Martin** and **Marchoux**, Professors at the Pasteur Institute, did not fail to evoke this dogma to cast suspicion on **Portier's** experiments.

I criticized their report on page 203 of this book and demonstrated that the three conclusions they drew are false and unacceptable. I demonstrated that the conditions under which they carried out this control are most unfavorable to the manifestation of the truth.

On this occasion, I recalled **Galippe's** control experiment, the aim of which was to establish the proportion of errors that accidental contamination by atmospheric germs can cause in experiments exactly similar to **Portier's**, but relating to plants. He inoculated 79 tubes with sterilized pumice stone fragments, contained in a single bottle that he had to uncork 160 times to collect the inoculated fragments. Despite this, none of the 79 tubes appeared soiled after three months.

Although this peremptory, absolute demonstration was carried out in 1887, this did not prevent Messrs. **Martin** and **Marchoux**, who were the objectors, from invoking contamination by atmospheric germs in their report in 1918. This demonstrates that, for the protectors of pasteurian dogmas, demonstrations are useless; the progress of science does not count for them; only the maintenance of false dogmas counts, despite everything and against everything.

II. THE FALSE DOGMA OF THE ASEPSIS OF LIVING ORGANISMS

The history of the development of this dogma is intertwined with that of the preceding dogma. It is that of the origin of the alcoholic ferment in grape juice. **Pasteur** never wanted to admit the normal existence of ferment in the grape berry; he maintained all his life, against the evidence, against all the formal demonstrations that were opposed to him, that the ferment is contained in the air and that it is because it comes to be deposited on the surface of the grape berries that their juice undergoes alcoholic fermentation.

Likewise, throughout his life he stubbornly contested the accuracy of **Turpin's** observations (80) on the effusion of "seminuliferous globulins" from brewer's yeast globules and even the existence and evolution of these globulins; this was a fact so easy to verify, for a man as knowledgeable as **Pasteur** on such a subject, that one is obliged to admit that this stubborn denial, this refusal to accept the truth, was dictated by an extrascientific motive which will be discussed later.

This obstinacy in supporting the atmospheric origin of ferments and contesting their intraorganic origin led **Pasteur** to the false dogma of the asepsis of the organism of living beings and to admit that **the body of animals is closed, in ordinary cases, to the penetration of germs from inferior beings***;* it led him to the false conceptions that he expounded in his 1853 memoir on putrefaction and which provoked the lively discussions he had with **Béchamp** on the subject of the fermentation of meat, blood, and milk.

* * *

It is likely that **Pasteur's** obstinacy in denying the evidence was dependent on his religious convictions, as Professor **Rappin** (71), director of the Pasteur Institute in Nantes, explained in the following passage from his very remarkable book on the etiology of infectious diseases (71, p. 10):

Pasteur *himself, with his natural ardor and impetuosity, did not fear, in order to defend his experiments, and even their scope, to enter directly into the fray, not only from the point of view of his work in its relationship with science, but also in the consequences that he sought to draw from it from the point of view of his religious ideas. In a lecture given at the Sorbonne, he even attacked the so-called materialism of the partisans of heterogeneity:*

What a conquest, he said, gentlemen, for materialism, if it could protest that it is based on the proven fact of matter organizing itself, taking life of itself! What could be more natural, then, than to deify this matter? What good, he said, is there in resorting to the idea of a primordial creation before whose mystery one must bo*w?*

And, appreciating this conference, an abbot, Father **Moigno**, wrote:

*It was a question of winning over unbelievers and materialists to spiritualism, and **Mr. PASTEUR** had confidence in his mission. He felt that he had charge of souls.*

*He was praised, even from the pulpit of Notre-Dame, by **R. P. Félix**, for the orthodoxy of his chemical doctrine…*

Professor **Rappin** then adds:

*Also, all these discussions bore fruit, and it was under these conditions that the protagonists and the triumvirs of spontaneous generation, as one author of the time, **Victor Meunier**, called them, the **Pouchets**, the **Jolys**, the **Mussets**, were the object of real persecution from above, and were even, something barely believable, cited in court for the freedom of their opinion*.

*We are fortunately far from the time when people seemed to be trying to revive the funereal agitations that, as history teaches us, led to the torture of **Giordano Bruno** and forced **Galileo** to deny his own ideas. But let us not be mistaken; if, in truth, after so many struggles, freedom of research and thought now seems to be a little better protected, it is nevertheless far from being possible for researchers to remain in the fullness and independence of their personal ideas and their thoughts and are therefore called upon to clash with certain convictions.*

*It is this same spirit, moreover, that we saw manifest a little later within the Academies, in particular when **Pasteur**, attacking **Béchamp**, passionately and often even cruelly fought against this scientist's ideas on microzymas.*

Chapter XIX of this volume, entitled: *The Strangulation of Freedom of Opinion and Publication in France***,** is a natural continuation of Professor **Rappin's** opinion.

It will inform the reader about what freedom of opinion is in scientific circles, on the catastrophic influence of so-called learned societies on scientific progress and on the decline of freedom of thought, conscience and morality in the intellectual class, a decline of which certain recent events of the last war are only one aspect.

* * *

Religious reasons and the fight against materialism therefore intervened in **Pasteur's** determination to deny the normal existence of bacteria in living beings. This is a narrow point of view and an error.

I protest against the habit of opposing materialism to ideology. Scientific materialism is not what is generally thought; it must be defined: the determinism of the mechanism of all phenomena and their causes.

Scientific materialism cannot not exist, for it is the necessary basis of all human progress. It is in no way opposed to ideology in general and to religious ideology in particular, firstly because it singularly enlightens the mind and, secondly, because there are regions of science that it cannot explore: for example, the origin of matter, the infinity of space and time, things which, at least at present, are beyond the reach of human understanding.

Moreover, the bacterial nature of living organisms, the evidence of the bacterial nature of the first living beings, the total return of living matter from organized beings to bacterial dust and finally to mineral dust, is much better in accordance with religious principles than the dogma of the asepsis of living organisms.

Finally, the question at stake is not whether the bacterial nature of living organisms is in accordance with religious principles or not, but only whether it is indeed the expression of the truth. No religious principle can be opposed to the search for this truth, knowledge of which is essential to determining the original source of the viruses of human diseases in order to protect us against the scourges that afflict us-the plague, cholera, smallpox, diphtheria, poliomyelitis, tuberculosis, cancer, etc. and to eliminating the physical and moral suffering they cause. Can one imagine a higher ideology than the determinism that leads to such results?

Faced with this need to know the truth to avoid these scourges to humanity, let us note that **Pasteur**, whether for religious or other motives, introduced false dogmas and principles into science which, first, stifled the truth that had been in progress since the beginning of the last century, then made it impossible for researchers, right up to the present day, to find it by directing their work in the wrong directions, a harmful operation that the pasteurian school actively continues against all new notions opposed to these false dogmas, but which they want to keep inviolable nonetheless.

Here, to support these assertions, are facts that peremptorily demonstrate the falsity of this harmful dogma of the asepsis of living organisms, which the new knowledge of the colibacillary function definitively reduces to nothing on its own.

The phenomena of putrefaction

The scientific world, in France and abroad, still believes, at the present time, in the pasteurian dogma of the asepsis of the organism of living beings. It still believes that, as **Pasteur** affirmed, the putrefaction of an animal's corpse begins at the surface of the body and the intestinal canal to gradually reach the interior of the solid mass and that it is caused both by atmospheric germs and by bacteria in the intestinal canal.

One might believe, because of this universal conviction of hundreds of thousands of scientists and physicians from all nations of the world, that it is established on sound, material, and unassailable scientific bases. This would be an error.

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These bases are the gratuitous, erroneous assertions that led **Pasteur** to his theory of atmospheric panspermia and to conclude that germs suspended in the air are the source and origin of organized ferments; they are the false, inadmissible assertions that **Pasteur** formulated in his 1853 memoir on putrefaction (57) and in the memoirs he published later on the fermentation of blood, milk, and urine.

It is not possible that the vast majority of scientists know exactly the value of the principles that are the basis of their conviction, because examination of these quickly reveals their falsity; therefore it is necessary to make these bases, which have fallen into oblivion due to their remote origin (1853), exactly known. I believe it is necessary to reproduce here first the final part of **Pasteur's** memoir on putrefaction. Here it is:

L. Pasteur, C. R. Acad. des Se., t. LVI, p. 1193...

Let us now consider the putrefaction of solid substances. I have recently proven that the body of animals is closed, in ordinary cases, to the introduction of germs from lower animals; consequently, putrefaction will first establish itself on the surface, then it will gradually spread to the interior of the solid mass.

As for an entire animal, abandoned after death, either in contact with or sheltered from the air, the entire surface of its body is covered with dust carried by the air, that is, with germs of lower organisms. Its internal canal, especially where fecal matter is formed, is filled not only with germs, but with fully developed vibrios that ***Leuwenhæck*** *had already noticed. These vibrios are far ahead of the germs on the surface of the body. They are in the state of adult individuals, deprived of air, bathed in liquid, in the process of multiplying and functioning. It is with them that the putrefaction of the body will begin, which has been preserved until then only by the life and nutrition of the organs.*

Such is, in various cases, the progress of putrefaction. All the facts I have listed will be presented in the memoirs I will publish later, with all the experimental evidence they contain, but these facts could be misunderstood or misinterpreted if I did not add a few developments that the Academy will undoubtedly excuse.

To clarify the idea, let us consider a voluminous mass of muscular flesh: What will happen if we prevent external putrefaction? Will the meat retain its condition, structure, and quality from the first hours? We cannot hope for a partial result Indeed, it is impossible, at ordinary temperatures, to protect the interior of this flesh from the reaction of solids and liquids with each other; there will always and inevitably be so-called contact actions, diastasis actions (if I may be allowed this expression) which develop in the interior of the piece of meat small quantities of new substances, which will add their own flavor to the flavor of the meat. Many means can oppose the putrefaction of the superficial layers. It is enough, for example, to wrap the meat in a cloth soaked in alcohol and then place it in a closed vessel (with or without air, it does not matter), so that the evaporation of the alcohol vapors cannot take place. There will be no putrefaction, either internally because the germs of the vibrios are absent, or externally because the alcohol vapors oppose the germs on the surface, but I have observed that the meat rots in a pronounced manner, if it is in small quantities, and that it becomes gangrenous if it is in larger masses.

In my opinion, this is one of the examples where the ordinary definition of putrefaction is too broad; there is no similarity of nature or origin between putrefaction and gangrene. Far from being putrefaction properly speaking, gangrene seems to me to be the state of an organ or part of an organ preserved, despite death, protec- ted from putrefaction and whose liquids and solids react chemically and physically, outside of the normal acts of nutrition (1).

* * *

Thus, here is the memoir which, with the gratuitous and false affirmation of the atmospheric origin of the germs of organized ferments, has established the worldwide conviction of the accuracy of pasteurian dogmas and **Pasteur's** conclusions on the putrefaction of corpses and solid organic matter.

This worldwide conviction is established:

1. By a memoir containing only gratuitous and false opinions or affirmations, not a single one of which is supported by an experimental fact, and in which one does not find the slightest attempt at determinism;

(1) *Death, in other words, does not suppress the reaction of liquids and solids in the orga- nism. A sort of physical and chemical life, if I may so speak, continues to act. I would dare say that gangrene is a phenomenon of the same order as that offered to us by a fruit that ripens outside the tree that bore it.*

2. By a memoir in which it is affirmed that the gangrene of meat left to itself and the fact of going stale, has no similarity in nature or origin with putrefaction;

3. By a memoir in which the author, after having explained why the gangrene of meat seems to him to be far from putrefaction properly speaking, assimilates this phenomenon of indisputable rot to a phenomenon of the same order as that which a fruit which ripens outside the tree which bore it offers us;

4. By a memoir affirming that inside a large piece of meat left to itself there will be no putrefaction because the germs of vibrios are absent, whereas on the contrary:

A) There exist billions of bacteria which are the micrococcal form of the colibacillus.

B) Putrefaction always takes place after 4 to 6 days at an average temperature of around 20°C and is accompanied by the development of bacteria, as demonstrated by **Servel's** experiment, the consistent result of which is beyond all criticism;

5. By a memoir in which a very knowledgeable chemist limits himself, instead of chemical precisions, to predicting modifications in a block of meat by invoking the reaction of solids and liquids on each other, and "so-called contact" actions, which he does not specify, and have no meaning; memoir in which he writes, without providing any precision:

There will always and inevitably be so-called contact actions, diastasis actions (if I may use this expression) which develop small quantities of new substances inside the piece of meat, which will add their own flavor to the meat.

This single expression with the actions of diastases demonstrates that this writing by **Pasteur** is pure imagination; it is in formal contradiction with an absence of vibrio germs in the interior of meat and shows that **Pasteur** was using a term whose qualities or properties were very vague in his mind; in any case, this term condemns his entire argument, because a ferment or diastase can only act through the granular and micro coccal figurative elements that it contains and which are of a bacterial nature.

How, in the face of such a mass of gratuitous, imaginary assertions, from which one cannot derive a single exact objective notion, could a worldwide conviction have been established? It is exclusively through cleverly organized propaganda, which has managed to exclude and stifle notions contrary to dogmas and all contradictions.

However, at the time of **Pasteur's** publications, contradictions were not lacking. It was **Frémy** who relentlessly attacked the dogma of atmospheric panspermia, then **Béchamp** who, through numerous experiments, pursued for more than twenty years and executed

with precise determinism, reduced to nothing all of **Pasteur's** conclusions relating to the putrefaction of corpses, meat, blood, milk, and urine.

Through the following experiment, **Béchamp** demonstrated the formation of bacteria in meat aseptically extracted from the body:

He introduced a fairly large piece of meat, preserved in creosote water, into a flask containing starch paste that had been boiling for 30 minutes. The flask was then closed with a boiled stopper and placed in an oven. After 48 hours, the paste is liquefied and the entire mass is filled with small bacteria and moving rods. The following experiment by **Servei** (75, 1874), a student of **Béchamp**, is even more conclusive:

A dog is sacrificed by femoral hemorrhage; with the abdomen opened, a ligature is placed on the vascular bundle of the liver and kidney, then the organ, quickly and aseptically removed, is suspended by the ligature thread in a jar containing a 1% chromic acid solution and stored at a laboratory temperature of 15 to 20°C.

After five days, the organs are removed and examined; the surface is hardened and without the odor of putrefaction; when cut, they emit fetid odors.

On microscopic examination, the periphery of the organs is normal and without bacteria other than microzymas; the center, on the contrary, says **Servel**, is filled with bacteria animated by their characteristic swaying that the addition of the chromic acid solution immediately stops.

This experiment, which always succeeds, which is extremely simple, and free from criticism, is sufficient in itself to demonstrate the falsity of the dogma of the asepsis of living beings.

Its result formally contradicts **Pasteur's** assertion that, in a large piece of meat, "*there will be no putrefaction inside because the germs of the vibrios are absent*."

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Putrefaction having been demonstrated in such a piece of meat, both by the experiments of **Béchamp** and **Servel**, the reader will undoubtedly judge that **Pasteur** has gone beyond the limits within which interpretations must be maintained, in order to maintain a trace of plausibility, when he asserts that the fact of meat becoming stale or gangrenous has for it "no similarity in nature or origin with putrefaction."

Béchamp did not fail to qualify such assertions as purely gratuitous and imaginary, as did **Pasteur's** attempts to explain the stale smell and flavor of meat by *the reaction of solids on liquids, and by so-called contact or diastasis actions.*

I urge the reader to read the text of **Béchamp's** third and fourth lectures, in his book on microzymas (4). There he will see how he rigorously destroyed all of **Pasteur's** false conclusions on putrefaction and the supposed atmospheric origin of ferments. He formally demonstrated that the putrefaction or fermentation of meat and materials from living organisms is due to the previously known molecular granules, which possess fermentative power and which he called "microzymas."

It is such formal demonstrations, opposed to **Pasteur's** erroneous assertions, that a German author, **Altmann**, was able to describe, in his publication on"*Elementarorganismen*," as hypothetical interpretations of the processes of putrefaction. It must be believed that, to have been able to express such an opinion, he had not read or understood either **Pasteur's** memoir on putrefaction (1853), or **Béchamp's** memoirs (1860-1885), or his book on microzymas (1883).

CADAVERIC PUTREFACTION

Pasteur's theory to explain the putrefaction of corpses is inaccurate. According to him, "*the body of animals is closed, in ordinary cases, to the introduction of germs from inferior beings*..." This principle is not only false, but it is the negation of a function as crucial as the general bacterial function of all living beings, the colibacillus function in mammals.

Colibacillus therefore exists everywhere, in all parts of the organism. But, for **Pasteur**, bacterial germs only exist on the surface of the skin, coming from atmospheric dust, and in the intestine where they are in the adult state and in the process of multiplying. It is therefore through the latter and through the surface of the skin that putrefaction begins, according to him.

This conclusion is false; putrefaction begins everywhere since colibacillary cocci exist everywhere, but it is faster in some places than in others. Research carried out during the dying period has shown with certainty that the proliferation of colibacillus is already taking place at this time in all parts of the body (see: "Agonal Infections," Chapter 9). It is the agent of putrefaction in higher animals.

PUTREFACTION OR FERMENTATION OF BLOOD, MILK, AND URINE

Contrary to evidence that was already evident at the time of his discussions with **Béchamp**, **Pasteur** denied the fermentative role of microzymas, and even their presence, since he wrote that muscles and fresh blood do not contain bacteria, even though they contain an innumerable quantity of living micrococcus, which are microzymas.

Lively discussions took place between **Pasteur** and **Béchamp** on the subject of the putrefaction or fermentation of blood, milk, and urine. Pasteur maintained that these liquids are sterile and only putrefy if they are soiled by atmospheric germs.

PUTREFACTION OF BLOOD

For blood, direct examination of the clot shows an innumerable quantity of colibacillary cocci (microzymas); on the other hand, the inoculation in broth of the white layer which

surmounts the red blood cells of centrifuged blood, collected aseptically, develops a culture of colibacillus; microscopic examination of this white layer shows that it contains the bacillary and micrococcal forms which develop the culture.

The layer forms a culture of colibacillus; microscopic examination of this white layer, which only shows the normal presence of the bacillary form of colibacillus and its micrococcal form in normal blood as it leaves the vessel, condemns, beyond any possible discussion, all of **Pasteur's** argument to show the state of asepsis of the blood in the organism.

Pasteur wrote about an experiment in which he received dog blood in a flask connected directly to a vessel by a tube with a tap that was then closed:

Blood does not putrefy, even at the highest atmospheric temperatures; its odor remains that of fresh blood, or takes on the odor of laundry detergent. Contrary to what one might have believed, the direct oxidation of the principles of blood, its slow combustion, is not very active; after exposing the flasks in an oven at 25 or 30°C for several weeks, an absorption of 2 to 3% of oxygen is still observed, which is replaced by a roughly equal volume of carbonic acid gas.

Let us first note that this last statement is inaccurate: arterial, oxalated blood, from dogs or other animal species, collected aseptically in sealed, full tubes and placed in an incubator at 30 or 37°C, loses its oxygen fairly quickly; while it contains on average 17 to 18% upon leaving the vessel, this figure falls to zero in 24 to 48 hours.

Oxygen disappears from the blood extremely slowly only if, after being heavily centrifuged, it is placed in an incubator in this state, the globules remaining separated from the plasma. Let us then note that, receiving the blood in the presence of a certain volume of air, the assessment of the quantity of oxygen lost could no longer be accurately assessed, and that this fact is the cause of the error in the aforementioned conclusion. **Pasteur** then wrote about this experiment:

Under the circumstances I have just described, where dog blood exposed to contact with pure air does not putrefy at all, blood crystals form with remarkable ease. From the first days of its exposure to the oven, slowly at ordinary temperature, the serum gradually turns dark brown. As this effect occurs, the blood corpuscles disappear, and the serum and the clot fill with very distinct crystals, tinted brown or red. After a few weeks, no blood corpuscles remain, either in the serum or in the clot; each drop of serum contains thousands of these crystals, and the smallest particle of clot, crushed on the surface of the nail, yields colorless, very elastic fibrin, associated with clumps of crystals, without the slightest trace of blood corpuscles being discovered anywhere If we wait even longer, it sometimes happens that all the fibrin gathers into a single hyaline mass, which has gradually expelled all the crystals from its interior.

Before setting out my own criticism of this experiment, I will first reproduce here a passage from Béchamp's (Les microzymas p. 262):

... If Mr. ***Pasteur*** *had pushed the analysis further, he would have found other products of fermentation. The author did not see any bacteria there, but we know that he did not see any in the meat that is rotting, as he puts it. I am quite happy that Mr.* ***Pasteur*** *did not see long bacteria, the kind that everyone can distinguish; but he neglected, because he did not see them, or because he regarded them as meaningless, the molecular granules, isolated or coupled. When Mr.* ***Pasteur*** *says that the blood is not putrefied, he is paying lip service, as when he said that meat rots through the actions of diastases. I am quite willing to admit that in the altered blood of his experiment there is no hydrogen sulfide, scatol ammonia hydrosulfur, or other odorous or foul substances, but that is not enough to affirm that there are no fermentations of another kind. It is with this levity that Mr.* ***Pasteur*** *concludes in such a serious matter: he acted in this circumstance exactly as in his studies on milk, on meat, and, we will have to come back to this, on urine. In short, without suspecting it, Mr.* ***Pasteur*** *verified the microzyma theory. It is the microzymas of the blood that brought about the transformations that Mr.* ***Pasteur*** *noted and which he was unable to explain.*

This criticism is perfectly accurate, but incomplete. It must be supplemented by the following observations:

1. The fermentative action that occurs in the blood, immediately upon its exit from the vessel, is attested by the formation of the fibrin coagulum which results from the fixation, on the fibrinogen, of the acid fraction of blood lecithin, detached by the fermentative action of the fibrin-ferment, that is to say, the colibacillary cocci;

2. The conditions of **Pasteur's** experiment, in question here, were poorly chosen to establish whether the blood putrefies or not. Indeed, the coagulation of blood has the

effect of **immobilizing** in the clot the largest part of the micrococcal granulations of the blood by the fibrin filaments which attach themselves to them and suppress their Brownian motion. Now, it is these granulations which cause cadaveric putrefaction.

If we want to check whether the blood is putrefying or not, we must therefore operate with blood made incoagulable by a substance which does not suppress the fermentative power of the colibacillary granulations, nor their power to multiply actively, such as, for example, sodium oxalate, used at a low concentration. Under these conditions, even in the oxalated plasma alone, isolated from the blood cells, long chains of colibacilli are formed. This evolution of the colibacillary granulations into mycelial filaments which segment into bacilli definitively settles the question; the blood putrifies because it normally contains the elements of putrefaction which are those of the colibacillary function;

3. Such a discussion was undertaken primarily to determine whether blood is aseptic or not; now, to resolve this question, it was sufficient to centrifuge oxalated blood in a tube about ten centimeters long, well filled, to take a small quantity of the light whitish layer which surmounts the red blood cells, then to examine it under a microscope after having deposited it on a slide, spread and colored with dahlia.

Under these conditions, we observe in the blood the presence of colibacillary granulations, that is to say micrococci, then bacilli of various lengths which are colibacilli and finally white blood cells and hematoblasts (platelets) which are agglomerations of colibacillary micrococci.

All these elements normally exist in the blood because they are those of the colibacillary function of mammals or the bacterial function of living beings.

Béchamp also demonstrated precisely that, in blood deprived of contact with air, the latter being replaced by carbonic acid, numerous bacteria appear.

The question of the fermentation or putrefaction of blood is thus settled. It was **Béchamp** who observed the exact facts and **Pasteur's** assertion was a big mistake.

Moreover, the so-called aseptic autolysis of tissues, well known today, makes it possible to explain the phenomenon of blood autolysis inaccurately presented by **Pasteur** as an aseptic autolysis; this phenomenon is a bacterial autolysis, carried out by the organic bacillus coli, that is to say by the fibrin ferment of the blood.

This phenomenon of tissue autolysis is described in the chapter on the nature and origin of ferments, where we can see that it is carried out by a ferment that has exactly the same proteolytic, glycolytic, and lipolytic properties as fibrin ferment or colibacillary ferment.

This phenomenon of autolysis has been called "aseptic" to bring it into line with the dogma of asepsis in the animal organism; it is therefore by this dogma that its nature and meaning have been distorted. But the fact that it is admitted without dispute that it is due to the action of diastases restores its bacterial nature.

COAGULATION, FERMENTATION, AND PUTREFACTION OF MILK

Béchamp clearly demonstrated that, contrary to **Pasteur's** assertions, the coagulation of milk and its butyric and lactic fermentations are due to the microzymas that it normally contains. No one could now dispute that the granulations (microzymas) that milk contains are the active agents of the lactic and butyric fermentations that it undergoes; these granulations are the colibacillary cocci of the blood that have passed through the mammary gland.

A piece of curdled milk, crushed between two glass slides, then fixed by heat and colored with dahlia, shows, at a magnification of about a thousand, an innumerable quantity of granules, some of which are provided with a short filament emitting fine, short-pedunculated granules which are zymospores; this is the mode of multiplication of fer- ments. On the other hand, a piece of curdled milk, being transported in peptone-glucose broth, cultivates there by developing first chains of cocci (streptococcus) and some bacillary elements, then, in 5% saline peptone solution, exclusively bacillary elements which are colibacillus and also *Aeschrich's Bacillus lactis acrogene*s, included by bacteriologists in the series of colibacilli.

PUTREFACTION OF URINE

It was studied in a precise manner by **Béchamp** who, at the beginning of his study, wrote (Les microzymas, p. 706):

*It is necessary to make known the observations of Mr. **Pasteur**, because he has introduced into science, on this subject as well as on so many others, such serious errors that they must be resolutely combated.*

Here are **Pasteur's** observations, summarized:

Examining the deposit that forms at the bottom of a flask of urine exposed to the air, he found a torulaceae in strings of very small grains, whenever the liquid had become ammoniacal by the transformation of urea. For **Pasteur,** this torulaceae would be a ferment brought by the air and intended to destroy the urea. To prove this, he carried out the following experiment:

He introduced, into a flask containing filtered and boiled urine and sterile air, a wad of asbestos which had been exposed for several hours to a current of air. After seven days, he noted that:

The liquid is very noticeably alkaline to red litmus paper; however, the alkaline reaction as well as the action of hydrochloric acid indicate that not much ammonium carbonate has yet formed.

The liquid also contained:

Small bacteria, several of which were still very agile, and very small monads that moved in curves; there was also the torulacea with small grains gathered in the form of short strings.

Béchamp responded to this experiment by saying:

Yes, germs in the air can play a part in the phenomenon of urine putrefaction, but they are not necessary.

He first showed that, among various samples of urine collected without being filtered and protected from the air with the necessary precautions, some remain acidic and contain torula, while others, having become alkaline, do not.

On the other hand, **Béchamp** noted that urine contains microzymas. He wrote, (Loc. cit. p. 709): On this subject,

When one wishes to fruitfully study the microzymas of urine, one must proceed as we have always done until now: slightly creosoted or carbolic urine, at one or two drops per 200 cubic centimeters, as it leaves the urethra, is examined at intervals of one or two days or more often. One then sees the microzymas appear in the form of two joined spheres, representing a figure 8; then the number of grains increases and one has straight or sinuous strings of three, four, and a greater number of grains: the torulacea of Mr. ***Pasteur*** *and Mr.* ***Van Tierhem****; rarely, under these conditions, one sees the bacteria appear* **(1)***.*

If the urine has not been carbolic, one must operate more often. One observes the same evolutionary phenomena: the microzymas come together, multiplying, in the form of strings of grains, then they lengthen and the bacteria appear Sometimes it is a sort of small vibrio that precedes the bacteria. I have often seen all the isolated microzymas disappear and obtained only small chains.

The entire evolution of urine putrefaction is contained in this quote. But it lacks the details that **Béchamp** could not give at the time of its publication. These details are here:

(1) *By this word Béchamp designates the bacillary form.*

I examined numerous samples of urine, collected with the necessary precautions and protected from the air. Here is what the examination, extended until the urine was completely putrefied, showed me:

1. The urine leaving the bladder always contains numerous mobile granulations, which are the colibacillary cocci of the blood (microzymas) carried by the urine; they are of the same nature and origin as those carried by milk, digestive juices, various glandular secretions and pathological liquids which appear in the serous membranes;

2. After a variable time, the urinary colibacillary cocci form, as they evolve, germinative masses giving rise to mycelial filaments which segment until they reach the state of granulations remaining attached. This evolution of germinative masses and filaments is extremely rapid, and these are rarely seen again adherent to the masses which formed them.

In certain chains, the segmentation appears, in places, in the state of coccobacilli or very short bacilli which, segmenting late, form diplococci.

These chains of cocci are the torula, described by **Pasteur** and **Van Tieghem**, and have nothing in common with the species of hyphomycetes of the genus torula.

They are simply the streptococcus and are identical to the chains of *Streptococci* that develop in milk or in broth inoculated with a particle of curdled milk, also identical to the chains of cocci that develop either in urine left to itself or in blood serum preserved at room temperature away from the air, and identi- cal, finally, to all the chains of streptococci that can be found in various parts of the body. All these chains are the same evolutionary form of the normal colibacillary cocci of the blood, a fermentative organelle of the living organism (fibrin-ferment) and an element of a bacterial nature;

3. The evolution of the colibacillary cocci being pushed further in the urine which putrefies, the mycelial filaments produced by the germinative masses become more robust and segment, no longer into granulations, but into bacilli which are the bacillary form of the colibacillus.

By inoculating in peptone solution small chains of streptococci developed in broth by inoculating a piece of curdled milk, they were transformed into the bacillary form which is *Bacillus lactis aerogenes* (*Aeschrich's bacillus*) which is exactly the same colibacillus as that which develops in urine in advanced putre- faction. Both originate from the colibacillary cocci of the blood, and they present only very slight differences between them due to the nature of the liquids which carried the cocci, or to the different action of the glands which they crossed. The bacillary form develops moreover directly after the small chains of cocci, in broth cultures inoculated with a piece of curdled milk;

4. In an even more advanced stage of putrefaction, a gelatinous mycelial veil forms on the surface of the urine, the mycelium of which is identical to that which constitutes the white flakes which appear in the oxalated and phosphated plasma in the process of slow coagulation (p. 117, 1-; vol.).

* * *

These observations therefore demonstrate:

1. That **Pasteur** erred in attributing to contamination by atmospheric germs the origin of the *Torula* whose presence he observed in urine in the process of putrefaction;

2. That this *Torula* does indeed have as its origin, as **Béchamp** concluded, the molecular granules long known that he called microzymas and which are the colibacillary cocci of the blood;

3. That the evolution of the putrefaction of urine is exactly as B**échamp** described;

4. That the presence of colibacillus, following the chains of colibacillary cocci in the urine that putrefies, is a normal consequence of the bacterial function of the organism and that it does not come from a passage of intestinal colibacillus into the urine via the blood;

5. That urinary colibacillosis does not result from an infection of the urine by intestinal colibacillus that would have crossed the mucous membrane of the intestine and infected the blood. The germ of urinary colibacillosis normally exists constantly in the blood and urine in the state of cocci; it is exclusively changes in the chemical composition and *pH* of the urine environment that determine the accidental evolution of colibacillary cocci in this liquid into the bacillary form. Those who are affected do not generally suffer from it and do not experience any discomfort; often they do not notice it until long after its onset.

A patient suffering from urinary retention of unspecified origin was followed by **Béchamp** from April 1871 to 1876. His urine contained a large quantity of strings of cocci and bacilli which, evidently, were colibacilli.

The patient had to catheterize himself several times a day until November 17, 1873. On that day, without any known cause, the evacuation of urine became spontaneous and the use of the catheter became unnecessary; however, the urine continued to contain

strings of cocci and bacilli; the fact was noted officially on July 11, 1876 by a commission of five professors from the Faculty of Medicine of Montpellier, who drew up a report in which it is noted that immediately after urination, whether emitted the day before, had a frankly acidic reaction and contained free or associated molecular granulations two

frankly three to three, or in a string, and also contained a very large number of bacilli which are here called **bacteria**.

Béchamp followed the patient until 1880 and, at that time, there were still bacilli and strings of cocci in the urine.

This is a case where the simultaneous presence in the urine of isolated and normal colibacillary cocci, and their forms of development: string of cocci (streptococcus) and bacillary form (ordinary colibacillus) persisted for at least nine years and became a state that can be considered normal, not manifesting itself in the subject by any particular disorder.

From the above, it also follows that the action of the numerous anti-colibacillary vaccines currently available to doctors can only be completely null against urinary colibacillosis, as well as against all diseases accompanied by abnormal colibacillary vegetation, this because it is impossible to vaccinate against a normal organelle which is one of the two constituents of the animal organism and which exists there in quantities of hundreds of billions.

Anti-colibacillary vaccines, and let us add anti-staphylococcal, anti-streptococcal, and anti-pneumococcal vaccines, are one of the regrettable consequences of the false direction given to scientific research by pasteur's false dogmas; in light of the new bacterial function of colibacillary, these vaccines appear as ridiculous, even grotesque, productions worthy of medieval medicine.

* * *

Note that, in all of **Béchamp's** research on putrefaction, he considers microzymas to be granulations of living matter, the molecular granulations formerly known, and refuses to consider them as bacterial in nature. He protests against the names micrococcus and microbes that some have given them. But, nevertheless, he recognizes their power to transform themselves by taking the form of strings or rods of varying length (bacilli). In this case, he says that this transformation has become possible because the microzymas are no longer normal, but have become morbid.

This childish explanation of **Béchamp** is not admissible because, in fresh and normal urine, the microzymas, which are perfectly normal, do not become morbid, no action intervening to cause this morbidity; they evolve to give rise to bacillary forms because this is their normal mode of evolution. **Moreover, the formal proof of the accuracy of this statement is that this evolution is normal in the blood of animals themselves, which always contains the bacillary form of the coli bacillus, and even the long filaments which segment into this bacillary form and are the product of the gorminative masses of the organic coli bacillus, which are the mononuclear leukocytes** (see plate 74, third volume, representing the normal elements of the dog's blood).

* * *

In summary, the following facts are intangible proofs, which demonstrate in a peremptory manner the falsity of the dogma of the asepsis of living beings:

1. The blood of higher animals normally contains the cocciform, bacillary and even filamentous bacterial elements of the organic colibacillus; the platelets (hematoblasts) are agglomerations of these cocciform colibacillary elements. All these elements are seen by centrifuging oxalated blood and examining under a microscope a little of the light white layer which surmounts the red blood cells; the colibacillary cocci are seen in innumerable quantities by examining, at high magnification, a small fragment of blood clot, crushed between two glass slides, then fixed and colored;

2. Fibrin ferment is made up of colibacillary cocci from the blood. Oxalated plasma and fibrinogen solutions purified by the addition of tricalcium phosphate, then left at a temperature of 5 to 20°C in a closed, aseptic container, slowly give rise to white flakes which are made up of balls of mycelial filaments, some of which are segmented into colibacilli;

3. Blood serum, collected aseptically and stored at laboratory temperature, contains, after a few days, chains of streptococci due to the development of colibacillary cocci;

4. The colibacillary cocci normally exist in countless quantities in tissues and organs, from which it must be deduced that if, when a tissue, liver, kidney, spleen, testicle, etc., is inoculated in broth, a bacterial culture is not obtained, it is because one did not know how or did not want to go about it properly, as I demonstrated regarding the control, by a Commission of the Biological Society, of **Portier's** experiments relating to "symbionts" (see p. 203).

Indeed, obtaining a culture of colibacillus is inevitable under these conditions because it is the agent of the bacterial function of living beings, a function as important and indispensable as respiration and blood circulation;

5. The colibacillary cocci in the blood are carried through the glands into the fluids that come out of them in the form of digestive juices, milk or urine. Those in saliva and the fluids emitted by the glands of the oral and nasal mucous membranes are the source of pneumococcus, staphylococcus and streptococcus, a fact that explains why the nasal and oral cavities are a normal habitat for these bacteria, just as the intestine is a normal habitat for the colibacillus, because it is continually introduced there by the digestive juices.

* * *

All these facts therefore demonstrate not only the falsity of **Pasteur's** dogma of the asepsis of living beings, but also that it has constituted, for three-quarters of a century, the negation of a function of living beings as important as respiration and blood circulation, the colibacillary function of mammals and vertebrates, a bacterial function, in more general terms, of all living beings, animals and plants.

One of the results of **Pasteur's** false assertions was to lead his school and all conformist workers who do not verify the accuracy of previously accepted facts to conclude that, whenever the presence of colibacillus has been observed in any part of the organism, abscesses, pericardial, pleural, or peritoneal fluids, or in a normal fluid such as urine, it is because the intestinal colibacillus has crossed the mucous membrane, passed into the blood, and then infected the affected region.

And all this is currently accepted as the pure truth, while the colibacillus, which is the active agent of the capital bacterial and colibacillary function, exists at all points of the organism, while it passes from the blood into the intestine with the digestive juices of which it constitutes the active elements, and is transformed there into these same vibrios that **Pasteur** considers as the agents which would putrefy the corpse by invading it after having crossed the intestinal mucosa, which is not at all necessary, since they exist at all points of the organism of the living animal.

This dogma is the one that has had the most harmful influence on the progress of medicine, bacteriology, and general biology, as well as on the fight against human diseases; the denial of the bacterial nature of living beings is notably the cause that has led therapy to antibacillary and especially anti-colibacillary chemotherapy, which, if it were truly active, would surely and quickly kill the sick, something that happens with salvarsan, for example, in fortunately rare cases, by destroying the colibacillary function and, moreover, the second bacterial organelle, the dumbbell organelle, builder of tissues and in particular of blood cells which, poisoned by this substance, disappear en masse.

* * *

III. THE FALSE DOGMA OF BACTERIAL MONOMORPHISM

It is the negation of the extremely developed polymorphism of living matter. It consists of attributing, to a bacterial form presenting certain characteristics and properties, the quality of a stable species; it does not accept bacterial evolution from one form to another, from the micrococcus to the bacillus form, and even less so, the transition from these two to the hyphomycete state.

This dogma is responsible for the ignorance of the evolutionary cycle of bacterial cultures, as well as the nature, mode of formation, and role of leukocytes, the germinal masses of in vitro cultures.

Since **Pasteur's** research, microbes have generally been considered to be the form of viruses, and it is because they were sought in this form that the viruses of certain diseases, such as eruptive fevers, infantile poliomyelitis, etc., ***could not be found.***

For almost all viruses of plant origin, the bacterial form is not the actual form of the original infecting virus but only a witness to the presence of this virus, a witness whose birth is subsequent to the start of the infection, but which is nevertheless capable of reproducing the disease.

For diphtheria, for example, the original virus that will constitute the false membrane is not the **Klebs-Loffler** bacillus, but the *Cladosporium herbarum* (or another conidial form) of cereals, and it is on the mycelial filaments of the already formed false membrane that the diphtheria bacilli are born.

This single fact of the birth of a bacillary bacterial form on the mycelial branches born from the spore of a hyphomycete constitutes the formal demonstration of the non-existence of monomorphism in bacteria.

I had already demonstrated this through research published in 1926 in the first volume of this work by transforming a very virulent anthrax culture into two forms of hyphomycetes, a *Penicilium* form and an *Aspergillus* form, then retur- ning the latter to the bacterial state by culturing its spores in broth. The spore emits, by germination, a large mycelial filament on which fine branches are born which quickly segment into bacillary elements which remain adherent to each other in long chains and which each have exactly the shape of the anthrax bacteridium. This transformation is shown by Figure 3 of Pl. 239 of the first volume, reproduced in Figure 1 of Plate 43 of the present third volume.

* * *

The dogma of bacterial monomorphism can be defined as follows: A microbial form, a bacillus, for example, is immutable; it indefinitely reproduces a similar bacillus, endowed with the same properties; it is a defined and fixed species.

This dogma is false. The form of a bacterial element, its biological properties, are only a passing, provisional state, a state that can be preserved for a fairly long time, but while maintaining absolute fixity to the constitution and physical conditions, humidity, temperature, etc., of the culture medium.

Despite these precautions, a major modification can occur in the culture during successive subcultures. It can pass from the bacillary state to the micrococcus state or, on the contrary, to the hyphomycete state.

It is also common for a bacterial culture transplanted onto agar and developed normally to pass, after a few weeks, to the hyphomycete state at the upper part of the tube where the agar thickness is the thinnest, because it dries out progressively at this point first if the tube is not provided with a rubber cap.

On the other hand, I demonstrated in 1926 (1st volume):

1. That bacteria can be transformed into hyphomycetes with ease and that these are easily returned to the exclusively bacterial state by culture in a liquid medium; that, even while retaining the form of a hyphomycete, certain mycelial filaments, crawling on the moist surface of solid media, segment into bacterial elements;

2. That bacterial elements have no individuality, and are only segmented elements of long filaments of living matter, or even of tiny particles of living matter in the process of multiplication or evolution;

3. That bacterial elements have no fixed form, and can easily pass from the micrococcus form to the coccobacillary or bacillary form, or vice versa.

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This fact has already been observed by **Hallller** (30), then by **Portier** (63) who saw that a bacterium can successively take the form of a micrococci, a bacillus or a long filament. **Portier** added, knowing that this statement was contrary to the accepted dogma of monomorphism: *Here is a statement that will arouse distrust in the minds of bacteriologists who will wonder if my cultures are pure. They are without any doubt, it is a point on which we will certainly never discuss.*

This great variability characterizes the extremely developed polymorphism of living matter. As a corollary, I also demonstrated that the conidial forms of hyphomycetes are also unstable and can switch from one to another, for example from the *Fusarium, Penorospora* or *Mucor* forms to the *Penicilium* form.

Finally, from 1925, **G. Enderlein** (17 A and 18 C) demonstrated that the forms of bacteria are only transient and represent only stages of the cycle of their evolution; that one form can easily pass into another; that a form which seems to have reached a definitive fixity and which he calls a state of mochlosis, can nevertheless undergo a new evolution if the necessary conditions are achieved in the culture; and he cites as an example the B. C. G. which, he writes, is in a state of mochlosis, but is likely to resume normal development and return to its primitive state.

In summary, **the dogma of monomorphism of the definite and fixed bacterial species is false; a bacterial form is essentially variable and never represents more than a stage in the evolution of the living matter from which it emanates and which is always that of an organized being, animal or plant.**

The mode of development or multiplication of bacteria is not the one currently taught

For the so-called sporeless bacillary forms, the mode of multiplication has never been precisely indicated or demonstrated. For the bacillary forms possessing spores, such as the anthrax bacterium, it is assumed that they develop through their spore. But the mechanism of this development has never been determined. For the granular forms, development by budding or division is assumed.

In reality, the development of bacillary forms always takes place through the four-stage evolution of all the elements of the culture, an evolution that I described and demonstrated in the first volume; these stages include:

1. The agglomeration of the elements into rounded masses of various sizes and their agglutination by a hyaline, achromatic matter developed in the culture;

2. The fusion of the elements;

3. The germination of the masses by the emission of long mycelial filaments;

4. The segmentation of the mycelial filaments into distinct bacillary elements, all equal or of variable length, elements which can segment in turn by dividing each time in half and thus forming shorter bacilli, then coccobacilli, and finally micrococci.

The same evolution occurs for the multiplication of the anthrax bacteridia. The germinative masses are formed by both granulations and agglomerated bacilli.

The same evolution takes place when it comes to the transformation of a hyphomycete into a bacterial culture, for example, that of *Cladosporium herbarum* from cereals into a culture of diphtheria bacillus. In this case, the conidia of the *Cladosporium*, inoculated in broth, germinate singly or gather in clusters which fuse into germinative masses, and these emit large branched mycelial filaments on which the diphtheria bacilli are born, either singly or in chains of two, three, or more (see plates 52 and 64 of this volume).

Thus, the same evolution takes place, even for the passage of a hyphomycete into the state bacterial.

The case we have to consider here is the multiplication of a bacterial culture by subculturing in sterile broth.

In such a culture, it is customary to consider the short bacilli as the youngest bacilli and to assume that they develop by elongating. This is also what **G. Enderlein** admitted in his studies (17 A). This is incorrect. The shortest bacilli always arise from the segmentation of bacilli twice as long (see p. 153).

In summary, **bacillary elements are not formed by the growth and elongation of a short element. They are always formed immediately, at their maximum length, by the segmentation of a long mycelial filament which, born from a germinative mass, is the only element that grows by elongation.**

As for the granular elements, they multiply in two ways. First, by division. I have never observed the multiplication of micrococci by budding.

Other elements, some more or less rich in chromatic matter, others achromatic and, among them, large spores of intrabacillary origin, germinate by emitting a fine and short divaricate filament which gives rise to several small, short-pedunculated basophilic granulations (3 to 6) which are zymospores.

This is the process of multiplication of the granulations which constitute the living ferments. This is the mode of formation of zymospores of the organic colibacillus (fibrinferment) (see p. 120, blood coagulation, 1st vol., and figs. 1 and 4, pl. 114, diastases).

In summary, the mode of formation and multiplication of bacteria that bacteriology teaches is inaccurate; for example, the anthrax bacterium would multiply by fissiparity and sporulation; it is neither by one nor the other of these two processes; the spores agglomerate with bacillary elements to form spherical germinative masses and it is these which, by germination, give rise to the long filaments whose segmentation will form the bacillary elements.

Contrary to the opinion of bacteriologists, the shortest elements of a bacillary-shaped culture **are not the youngest elements, destined to grow and elongate.** They never lie down; they always evolve by segmenting themselves, therefore by shortening. They are the oldest elements; the youngest elements are the longest elements.

* * *

The inaccuracy of monomorphism with regard to bacteria is thus demonstrated:

1. By the transformation of bacterial cultures into hyphomycetes when they are cultivated on a solid and not too humid medium and, conversely, by the return of hyphomycetes to the bacterial state when they are cultivated in a liquid medium;

2. By the possibility, for the same bacterium, of successively taking the forms of micrococcus, bacillus or long mycelial filament depending on the composition of the medium in which it is cultivated, the filamentous form being resolved into bacilli by segmentation;

3. By the fact that the same pure bacterial culture can be transformed into several different conidial forms of hyphomycetes, a fact demonstrated in many cases, for example for anthrax cultures, transformed into *Penicilium* and *Aspergillus*;

4. By the fact, demonstrated in the first volume, that even when the bacterium has transformed into a hyphomycete, its form is unstable and that its conidia form can spontaneously transform into another, for example, passing from the *Fusarium, Penorospora*, or *Mucor* forms to the *Penicilium* form.

* * *

Bacterial polymorphism includes three forms: the micrococcus form, the bacillary form, and the hyphomycete form, whose long mycelial filaments that segment into bacilli are the rudimentary evolutionary form.

Apart from these three forms, there are no others, the germinative mass, which in the blood is the leukocyte, the germinative mass of the organic colibacillus, being a stage of evolution and not a bacterial form

The spirillary form is not a bacterial form and cannot be classified into any genus or species.

One of the great errors of bacteriology is to consider spirillary forms such as Treponema pallidum, for example, as bacterial species. **Schaudinn's** *Treponema pallidum* has no individuality; it is only one of the spiral appendages carried in innumerable quantities by the mycelium of the hyphomycete which is the virulent agent of syphilis. I have demonstrated the accuracy of this statement by isolating this hyphomycete which I obtained by culture of the lymph, free of red and white blood cells, flowing from an indurated chancre which was extraordinarily rich in spirillae.

The mycelium of this hyphomycete bears an extraordinary quantity of spirillary

elements which appear to be of the same nature as the formations of *Trychophyton gypseum asteroides* which **Sabouraud** called spirals (see 1st vol., p. 622, and pl. 283 to 287).

The virulent agent of syphilis, the one which, by its vegetation, causes and constitutes the initial chancre and then, subsequently, the mucous plaques, the skin lesions, the ulcerations, the organ lesions, the gums and the lesions of the nerve centers, is therefore this hyphomycete and not *Treponema pallidum*, which is only its witness and an appendage, the mass of the canker being formed by the mycelium.

Cultivated in vitro, **Schaudinn's** spirilla cannot multiply directly in the form of spirilla, but only by agglomerating, with other elements, into germi- native masses which evolve in the form of the hyphomycete that I have described as the true virus.

It is the same for all true spirilla. Some have claimed to have obtained cultures of spirilla, something as impossible as harvesting plums without cultivating a plum tree.

I tried to obtain one of these cultures from bacteriologists; I was unable to succeed. On this subject, I must make known the following fact:

Having been invited to spend the day at the home of the owner of a major Parisian newspaper, I met there a distinguished professor from one of the great scientific institutes of Paris, who was dealing precisely with spirillosis in certain animals. I had recently published the first volume of this work. The conversation turned to this question and I made known what I have just explained above. My interlocutor assured me, in front of medical figures present, that he was obtaining cultures of spirilla. I replied that I believed this to be impossible but that, if he could entrust me with one of his cultures, I would undertake to publicly acknowledge my error. He accepted and it was agreed that I would send for this culture in his laboratory. The person who went there three days later for this purpose returned without the promised culture, but with a letter from this professor telling me that, because of the difficulty of maintaining these cultures, he limited himself, in his experiments, to inoculating spirillosis by injecting a few drops of blood taken from an infected animal.

This eminent bacteriologist had therefore chickened out. Of course, I didn't insist. I was convinced and, I add, quite unpleasantly impressed. I am now even more so for the following reason: The first volume of this work demonstrates that it is indeed, beyond all possibility of error, the true syphilis virus that I isolated, cultivated, described, and photographed in its various forms, **a virus previously unknown; this real virus, very easy to cultivate since I obtained it on my first attempt**, is of the utmost use for the study of the disease, to serve as an antigen and make the **Wassermann** reaction used for diagnosis more reliable, and above all, indispensable for the search for its original source, which, once known, would make it possible to definitively eliminate the disease, because **the**

first of all syphilis sufferers did not contract the disease from another infected individual, but from the original source, which is the organism of an animal or a plant.

And, despite this, none of those studying syphilis have dealt with this real virus; the two main institutes interested in this discovery have sought only to stifle it, no doubt reserving the right to publish it later under other names, something that will fool no one and cannot go unnoticed. Later, others will do the work of justice that I have completed here with regard to **Béchamp.**

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This discovery, dating back to 1926, demonstrates the absence of any concern for scientific progress and the general interest in these two Institutes.

Microbial Associations

It has become customary to judge the severity of infectious diseases based on the microbial associations observed, and when streptococcus is included, there is no shortage of indications of a worsening of the prognosis. However, it is known that the presence of streptococcus has been observed, alongside other bacterial forms, in frequent, very mild cases. Authors such as **J. Courmont** have already clearly noted that the fact that a bacterium appears in the form of a chain of cocci does not prove that it is a streptococcus.

It is the false dogma of microbial monomorphism that has forced bacteriologists to conclude that two bacteria of very different forms are two different species.

It is the lack of research on the evolution of bacteria, due to this dogma, that has prevented the error from being noticed and corrected. Indeed, to observe this error, it is sufficient to examine under a microscope the veil of a pure broth culture, a fragment of which is lifted, then carefully placed on a slide without disintegrating it, in the following two cases:

1. In the case of a bacillary-form culture, giving a veil that is in the process of passing to the hyphomycete form;

2. In the case of a culture of spores of a hyphomycete in broth.

In both cases, and at a temperature of 20 to 25°C, a veil is formed consisting of very large mycelial branches that are up to 6, 8, and 10 microns wide and which branch out into branches of increasingly smaller widths, 4, 3, 2, 1 microns, then 0.8, 0.5.

If the culture does not pass into the state of aerial hyphomycete, these branches segment into fragments, all of which, both those 10 microns wide and those only 0.5 mic-

rons wide, are bacillary elements. The finest, continuing their segmentation, manage to form chains of short bacilli, then coccobacilli, and finally cocci, these being streptococcus, according to the bacteriological definition, if they remain attached in a chain.

Since large, medium, and small bacilli, coccobacilli, cocci, and streptococci are part of the same individual, emanating from a germinative mass, and are formed from a common living matter of the same chemical composition, it follows that, despite their difference in form, there is an absolute identity between them, which explains why, through appropriate culture, any one of them can reproduce all the others; thus the falsity of the dogma of bacterial monomorphism is peremptorily demonstrated.

It is therefore impossible, in the presence of a mixture of diverse bacterial forms, to affirm that it is a microbial association. To affirm this, it would be necessary to isolate each of these forms and demonstrate:

1. That through proper cultivation, each of them cannot reproduce the others;

2. That each of them possesses pathogenic and fermentative properties different from the others.

The demonstration of the existence of considerable bacterial polymorphism in the living matter of an organized being, that is, the demonstration of the falsity of the dogma of monomorphism, can be made in a precise way, which does not lend itself to any discussion or contestation, by the following experiment:

Grind a few grains of barley into flour and spread the resulting flour at the bottom of a **Pietri** dish on sterilized, wet blotting paper; or, spread the black dust from the barley smut that covers the ears in the same way. After four or five days, several cultures of hyphomycetes of several different conidial forms will have developed; among them, we always manage to obtain *Cladosporium herbarum* of barley, an olive-green mold with a short lawn, very easy to distin- guish. Since it is not in contact with any of the neighboring molds, we subculture it two or three times on agar to be certain that it is pure, then we inoculate its spores into broth, which we keep at 25, 30, or 35°.

The various results that can be obtained are as follows:

1. Results photographed in plate 64, third volume. Groups of spores agglome- rated and gave rise to submerged colonies, as large as a grain of millet or a little larger, spherical, two of which are photographed in figure 1. They show radiating, branched mycelial filaments of varying diameters, bearing small protuberances on which characteristic diphtheria bacilli are born (figs. 2 to 8);

2. Other times, the broth quickly becomes cloudy (12 to 24 hours) then forms a veil which appears in the following various aspects (photographed in plate 65):

A) The veil is formed of large branched filaments (fig. 1, pl. 65) which emit others of two different diameters, some of 1 to 1.5 microns, others of about 0.3 microns, which segment into bacilli. The 1.5 micron bacilli segment into three or four micrococci, which separate, often remaining joined in pairs.

B) In another case (fig. 2, pl. 65) the veil remains formed of long, continuous or segmenting bacillary filaments, grouped into parallel filaments which become entangled in bundles in all directions and which are uniformly about 1 micron wide.

C) Other times (Fig. 5, pl. 65), the veil is formed, with the same constitution, by filaments uniformly 0.3 to 0.5 microns wide; in addition, the veil contains a certain number of branched mycelial filaments, 1.5 to 3 microns, which segment into very large bacilli.

D) In broth veils aged 8 to 10 days or more, a large number of micrococci can be observed, some in chains of 3 or 4, which are visibly bacilli segmented into three or four granulations (Fig. 3, gr. 800).

In addition, we observe mycelial filaments of 1 to 1.5 microns which segment into bacilli of granular appearance and whose granules are the spores. This bacillary form is observed quite often in diphtheria false membranes. We will see them in figures 2 and 5, plate A, and 3 and 4, plate D, showing the elements of diphtheria false membranes.

E) Sometimes, when the veil is not formed of large mycelial branches, it dissociates quite easily by placing it on a slide and it then shows (fig. 6, plate 65, 6-day veil, gr. 600) its very varied small, medium, and large bacillary elements.

F) A broth culture of the same type as the previous one, when subcultured on agar, can give a culture like that of Figure 7, Plate 65, showing medium and short bacillary elements, some of which have lost their chromatic material, except for the two polar globules which remain visible at both ends, a type very characteristic of the diphtheria bacillus.

G) Agar cultures of spores of *Cladosporium herbarum* from barley give rise, when they are old, to elements of a particular shape, characteristic of the diphtheria bacillus and which are called involution forms (fig. 8, 9, 10, 11, 12, pl. 65, 21-day culture).

It has been previously demonstrated that the diphtheria virus occurs in different conidial forms from wheat, barley, and rye and that its most common form is the *Cladosporium herbarum* form, that from barley appearing to be the most virulent. Thus, the vegetation of the same conidium, that of *Cladosporium herbarum* from barley, can give rise, by its germination:

- To very large bacilli whose diameter varies from 8 to 10 microns and to long mycelial filaments of the same diameter.
- Has bacilli 1 to 2 microns in size and long mycelial filaments.
- Has medium or fine bacilli 0.3 to 1 micron wide, and of all lengths.
- Has large granular bacilli 1.2 to 1.5 microns in diameter.
- Has micrococci of several sizes, often remaining stuck together in groups of 2, 3, or 4, depending on the length of the bacillus from which they emanate.
- Has bacilli with nothing stainable other than their wall and their two polar globules.

Almost all of these elements can be contained in the same veil and therefore do not constitute, in any way, a microbial association as bacteriology currently teaches; they are only different forms of the same individual, all combined at the beginning of its evolution and then gradually detaching from it.

Their union is therefore not a microbial association.

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The formal, peremptory proof of the accuracy of this conclusion is provided by the constitution of the false diphtheria membrane, the forming agent of which is precisely *Cladosporium herbarum* of wheat, barley, or rye, or another of their conidial forms.

Indeed, in Plate 52, whose photographs represent the constitution of a false diphtheria tracheal membrane (Figs. 1 to 7), we find all the elements of Plate 65: filaments of all sizes and large bacilli in Figures 1, 2, and 3; bacillary elements of all kinds in Figures 4, 5, 6, and 7; reproduction in Figure 8, in culture, of a large bacillary form from Figure 2, and we also find the conidia of the virulent agent (Figs. 3, 4, and 6, Plate 52).

In other diphtheria false membranes, other bacillary forms are observed, for example the streptococcal form in Figure 10, Plate 55, and in Figure 22, Plate 53. The short chains of the four micrococci in Figure 3, Plate 65 indicate that this form also falls within the category of bacterial forms of grasses.

Thus, the fact is demonstrated that the disparate forms observed in a false diphtheria membrane do not constitute a microbial association and that, on the contrary, they are constituent and integral parts of the same individual that they are all capable of reproducing in their entirety.

Thus, this dogma of the monomorphism of bacteria appears to be the biggest error in bacteriology.

A German scientist, **G. Enderlein**, who studied the evolution of bacteria for many years, concluded from his research:

Doctrinaire bacteriology is the expression of one of the most complicated systems of false conclusions that has ever mystified a science. The complete collapse of bacteriology based on monomorphism can no longer be concealed.

In reality, current bacteriology is not only a mystification, it is a deception, because it rests entirely on the three false dogmas mentioned above and is based on the biggest errors that any science has ever known, and which a category of scientists, aware of these errors, continue to want to maintain at all costs, against the interests of scientific progress, against the general interest, and for the sole benefit of particular interests.

IV. THE DOGMA OF CONTAGION

This dogma attributes the development of epidemics to the transmission of diseases by sick people to healthy individuals or to the absorption of food contaminated by the excrement of sick people.

Adherence to this dogma, along with those of atmospheric panspermia and the sterility of living beings, prevented bacteriologists from arriving at the concept of food-borne viruses and contamination through the ingestion of such foods.

Since **Pasteur's** research, viruses have been considered specific species that vegetate by perpetuating themselves on various organic materials, and whose origin is said to be very ancient. This is an error. A bacterial or hyphomycete species does not live long in nature where it does not find environments favorable to its preservation.

Viruses and all existing bacterial cultures originate exclusively from the organisms of living animal and plant beings, which spread them with their excrement or through the decomposition of their bodies after death. But this is not the origin of the viruses that contaminate humans and cause their heterogeneous diseases. These viruses are almost exclusively food, and particularly plant foods.

It has long been known that sweating sickness is not contagious. **Marinesco's** report (44) on the 1927 poliomyelitis epidemic in Romania indicates that, out of approximately 500 cases, there were no more than two or three cases in which contagion could be incriminated and that contagion had not occurred, even in cases where a child shared the same bed as a sick person.

The lesions of the nasopharynx indicate that they are the entry point for the polio virus. The statistics of the epidemic, showing that it is mainly children from six months to three years old who are affected, and those from six months to two years old in greater number than those from two to three years old, demonstrate that it is the children's very special diet, cereal porridge or cow's milk contaminated by bedding, which are the

source of the virus and that, consequently, it is through ingestion of the food-virus that contamination occurs.

The disease takes the epidemic form because the food-virus, introduced or existing in a given region, determines, through ingestion, the daily contamination of a certain number of children.

For diphtheria, the site of the lesion, the nature of the virus, *Cladosporium* and other conidial forms of cereals, as well as the age of the affected subjects, prove that it is through ingestion of the virus and not through contact with sick people that contamination occurs.

* * *

It is accepted that the plague is transmitted and spread by rats. It is known that when a plague epidemic develops, rats and other rodents are affected before humans, but this does not prove that it is the rat that transmits the plague to humans.

Nikanoroff's (53) study of the endemic plague focus in the Kirghiz country teaches us that the number of plague-infected ground squirrels, which is zero (0%) in April, increases progressively during the months of May (0.6%), June (5.8%), and reaches a maximum of 9% in July, at the time of maturity of a plant cultivated for its seed, called koumartchik (*Agryophyllum arenarium*; *Chenopodiaceae*). This fact demonstrates that if the plague were always transmitted among rats by contagion, or by fleas, it would continue to contaminate rats that remained unaffected after the proportion of infected reached 9%, and the epizootic would not end until all the rats were dead.

Nikanoroff's study further teaches us that plague epidemics in humans coincide either with harvesting and threshing, or with a plentiful harvest of Koumarchik, a fact known to the inhabitants who, says **Nikanoroff**, usually wait for the plague in the region where the harvest has been good.

The fact that the rat contracts the plague before the man does not mean that it is the man who contaminates it; it only means that the rat becomes contaminated before the man by ingesting the food-virus.

The fact that in April, not a single plague-infected rat was found out of 100 subjects, and that in July, at the time of the plant harvest, there were 9%, means that between these two dates, a virus, which is a rat food, developed. We cannot invoke, here, a pre-existing virus, and we are forced to admit that the infection of the first plague-infected rats occurs by direct ingestion of the developed virus and not by contagion.

* * *

All of these facts therefore demonstrate to us that the contamination of healthy individuals by the sick is not the cause of epidemics. In any epidemic, the sporadic cases at the beginning are formal proof of an infection that cannot occur by contagion and can only occur by ingestion of a virus-food. The demonstration of this fact is formal for diphtheria.

The dogma of contagion is therefore false, and it is abusive, unacceptable, to rely on it to make mandatory vaccinations that are harmful to the body and whose effectiveness has not been demonstrated by any evidence. The study of the diphtheria and tetanus viruses, made in this book, provides a striking demons- tration of this assertion by revealing the causes of the total lack of effectiveness of the vaccines used against them.

* * *

V. THE ERRORS OF BACTERIOLOGY

This is a succinct enumeration, which includes only brief explanations; a more detailed account is given for each case throughout this volume.

Here are these errors:

1. The false pasteurian dogma of atmospheric panspermia.

Contrary to **Pasteur's** erroneous conclusion, the germs of ferments and bacteria always come from an animal or plant organism of which they were a constituent part. The importance of germs living in the air, which is only the vehicle, for the contamination of culture media or organic matter, has been considerably exaggerated.

2. The false pasteurian dogma of the asepsis of living organisms.

The falsity of this dogma is demonstrated in several chapters of this book. Not only are living organisms not aseptic, but their living matter is constituted by two organelles of a bacterial nature, the dumbbell organelle which builds cells andtissues and the bacterial organelle (colibacillus in higher animals), which ensures the chemical reactions of the organism. Living matter is therefore essentially bacterial in nature.

According to this, it is useless to insist on the gross error of those who believe they have been able to create generations of aseptic animals by a so-called aseptic or asepticized diet.

3. The false dogma of bacterial monomorphism.

According to this false dogma, a micrococcus has nothing in common with a bacillus and is specifically different, the side-by-side coexistence of a cocci and a bacillus being a

microbial association. This is false. According to the general law of bacterial evolution, the germinal mass of a culture gives rise to a long filament which segments into bacilli, these in turn segment into smaller bacilli and then into micrococci. Micrococci and bacilli of different lengths, as well as long filaments and germinal masses, are therefore all differently shaped elements of the same living matter; they are not distinct species, and are only stages of evolution of the same mass of living matter in the process of multiplication.

This demonstrates that the notion of microbial associations, based on differences in the shapes of bacterial elements, is one of the biggest errors in bacteriology.

A single mass of living matter (germinative mass) can give rise to up to ten or more bacterial forms: filaments, bacilli of different lengths and sizes, with diameters varying from half a micron to 7 or 8 microns, coccobacilli, micrococci isolated or associated in pairs (diplococci) or in a chain (streptococcus). This fact is demonstrated on page 181 regarding the multiple bacterial forms to which *Cladosporium diphtheria* gives rise in the false membrane.

Another major bacteriological error is to consider that a chain of cocci or colibacilli is formed by originally isolated elements that then joined together in this form; this is false:

the chain is the state of a filament segmented into bacilli, or coccobacilli, or micrococci, which did not separate and remained attached; their previous state was the unsegmented filament and not the isolated and free form.

4. The mode of multiplication of bacteria explained in bacteriology treatises is inaccurate.

The general mode of multiplication of bacteria was explained in the first volume of this book and then, again, in the chapter "*Polymorphism of Living Matter*" of this third volume. The multiplication cycle includes four stages: Agglomeration of bacterial elements to form germinative masses, fusion of the elements, germination and emission of filaments, segmentation of the filaments and of the segmented elements themselves

5. Intestinal colibacillus is not a microorganism foreign to the body, as Pasteur concluded and as is still taught today.

It is the organic colibacillus of the blood (fibrinferment) passed into the intestine with the digestive juices in the cocci form and then evolved into the bacillary form. This fact explains why the typhus bacillus cannot be found in the intestine without it being associated with colibacillus and why, in newborns, colibacillus exists in its pure state in the intestine before any ingestion; it is brought there by the secretions of the digestive tract.

6. Contrary to Pasteur's conclusions, intestinal colibacillus does not cross the intestinal mucosa to cause putrefaction in corpses.

The conclusions of **Pasteur's** experiments on the putrefaction of corpses, meat, blood and urine are entirely false, contrary to the facts, result from a total absence of determinism (see p. 170). It was **Béchamp** who, at the same time, made a precise determinism of these phenomena and demonstrated rigorously, peremptorily, that the putrefaction of meat, blood, milk and urine is due to the granulations they contain, which he called microzymas and which evolve by giving rise to chains of granulations and to bacillary and filamentous microbial elements.

Béchamp's clear and rigorous demonstrations, opposed by **Pasteur**, were stifled and condemned to oblivion. This is one of the most incredible things imaginable, as well as the greatest injustice of the 19th century. It is not only **Pasteur** who is responsible for this stifling, which was catastrophic for the progress of science; it is also his successor as director of the **Pasteur** Institute who, throughout his life, by acting as the vigilant guardian of false dogmas, prevented all progress in bacteriology and encouraged the multiplication of the errors listed in this chapter.

The cause and origin of **Pasteur's** errors about putrefaction is the stubborn denial of the intraorganic origin of ferments and the stubborn affirmation of their atmospheric origin. He was forced, in order not to go back on his word, to chain together new errors following the first, notably the dogma of the asepsis of living beings, which led to all the erroneous assertions about putrefaction.

The putrefaction of animal corpses and plant debris occurs simultaneously at all points because they contain at all points the two bacterial organelles that constitute them, one of which is, for higher animals, the organic colibacillus.

7. Urinary colibacillosis does not result from the passage of intestinal colibacilli into the blood and then into the urine.

Organic colibacillus normally exists in urine, milk, digestive juices, and abnormal body fluids, in the same micrococcal form as in normal blood, where it is the fibrinferment. In each of these fluids, it can evolve into the bacillary form, if its constitution lends itself to it, and particularly if they are stored at room temperature.

Urinary colibacillosis is therefore due not to an infection of the urine, which normally always contains colibacilli in the micrococcal state, but to its evolution into the bacillary form and under the influence of a temporary modification of the chemical constitution of the urine.

8. It is Pasteur's false dogma of the asepsis of the animal organism that led to the creation as fixed and distinct species of colibacillus, Staphylococcus, Streptococcus, Pneumococcus, Enterococcus, Tetragenesis, Bacillus lactis aerogenes, Pneumo bacillus, Septic Vibrio, Legros' aerobic septic bacillus, Tetanus bacillus, Rhinoselerometa bacillus, etc., which are all colibacilli with very slight differences due to the constitution of the environment.

All these species, which have no real existence, are therefore not heterogeneous as bacteriology teaches, but normal and constituent elements of the organism they inhabit, or sometimes rendered abnormal by oxygen deprivation (tetanus).

This fact is one of the main ones proving that the current backward state of bacteriology is the result of the false dogma of the asepsis of living organisms and the false path it has taken scientific research.

9. The mouth, the nasopharynx, the digestive tract are the constant habitat of colibacilli, pneumococcus, staphylococcus, enterococcus, etc. because they are constantly brought there by saliva and the digestive and mucous glands, which themselves obtain them from the blood and not from the air and food.

10. It is false that toxins are a liquid acting through toxic and soluble microbial products.

They are the liquid part of a culture having carried with it, by filtering through a porcelain candle, all the granular elements of the culture of a dimension less than 0.7 and even 1 micron. It is through these figurative elements that it acts on animals and not through soluble toxins. Deprived of these figurative elements, by several successive additions of gelatinous tricalcium phosphate followed by centrifugation, the liquid becomes inactive.

11. It is false that a toxin (filter fluid from a culture) deteriorates if left in contact with oxygen in the air.

This is not an alteration; on the contrary, it is its natural evolution which takes place and which quickly reconstructs the bacillary forms of the primitive culture, the diphtheria bacillus if it is diphtheria toxin and the bacillary form of the human colibacillus if it is tetanus toxin. In the latter case, it is the colibacillus which appears because the tetanus culture is an anaerobic form of the organic colibacillary culture.

12. The horse's antitetanus serum does indeed contain an agglutinating antitoxin, but it is therapeutically inactive for humans and only active in animals in vivo and against the antigenic tetanus toxin, which it neutralizes in vitro and renders harm-

less to them because their organic colibacillus is specifically different from the organic colibacillus of the unknown animal from which the tetanus toxin originates.

(See on this subject the study of tetanus and antitetanus serum, Chapter XII).
Inoculation of tetanus toxin cannot cause immunization against human tetanus, because the tetanus virus is autogenous and a form of organic colibacillus, a major constituent of the organism. Claiming to immunize against it, even adulterated, is a misinterpretation.

13. Most of what we know about leukocytes: their classification, which does not respond to any special property for each category: phagocytosis, opsonins; the opsonic index, etc., are errors.

I have already demonstrated, in the first volume of this work, that leukocytes are the agglomeration masses or germinative masses of bacterial cultures, that is, a stage in their evolution, and I have shown them in in vitro cultures with their typical characteristics. This conclusion implied that animal leukocytes are the germinative masses of a bacterial culture vegetating in their organism.

The demonstration of the nature and constitution of the fibrinferment, its identification with the colibacillus, finally definitively establishing the bacterial nature of animal and plant organisms, also definitively established the nature of leukocytes: they are the agglomeration or germinative masses of the organic colibacillus culture in the blood.

They are made up of granules, which are the most common form of the colibacillus, the only form to which the fermentative function is attached. These granules are those to which bacteriologists have mistakenly given the names staphylococcus, streptococcus, pneumococcus, enterococcus, tetragenus, etc., depending on the appearance in which the mode of segmentation presents them. They are all colibacillus.

Depending on their state of development, colibacillary granules are basophilic, neutrophilic, or acidophilic, depending on whether they contain a lot, little, or no basophilic chromatic matter, and the agglomerated masses or leukocytes can contain both basophilic and acidophilic granules.

Colibacillary granules do not agglomerate in a special organ; they are present in the blood and everywhere. Their evolution involves the addition of bacillary-shaped elements, generally strongly basophilic.

The leukocytes in formation are initially acidophilic and composed of hyaline granules; their evolution is all the more advanced as they contain more basophilic chromatic matter. Their most perfect state is the entirely basophilic leukocyte, the mononuclear cell whose nucleus occupies the entire mass.

Their function is the formation of colibacillus elements by the emission of long, strongly basophilic filaments that segment into bacilli, which themselves manage to segment to the granular form. When successive segmentations have preserved the elements linked together, the last segmentation into granules constitutes the streptococcal chain.

In the constitution of the agglomeration masses between any micrococcal granulation; from this monopolization, by the leukocytes, of granulations considered by bacteriologists as staphylococcus or streptococcus, it was concluded that there was a phenomenon of phagocytosis or bacteriophagy and the secretion, by the leukocytes, of substances called opsonins which would cause the attraction of the granulations. These are phenomena of pure imagination.

All the granulations belonging to staphylococci, streptococci, pneumococcus, enterococcus, etc., assemble in leukocyte masses because they have a common specific link: they are the colibacillus.

As for phagocytosis, I established in the second volume of this work that it resulted only from an inaccurate hypothesis and from poorly observed and misinterpreted facts by showing that the supposed expansions of the lymphatic cells that constitute the nuclei of the giant cells and are supposed to seize and destroy the Koch bacilli contained in these cells (**Borel**) are precisely filaments of Koch bacilli; the cells from which they emanate have nothing in common with lymphatic cells: they are embryonic cells made up of dumbbell organelles, while leukocytes are not cells, being made up exclusively of colibacillary granulations.

The variation in the number of leukocytes in the blood, in certain diseases, corresponds to a change in the rate of multiplication of the colibacillus, that is, to a change in its evolution.

All these new notions show the multiple errors in the notions taught by bacteriology on leukocytes and their role. They are simply a stage in the evolution of the organic colibacillus.

They have fermentative properties analogous to those of the fibrinferment because the granules that constitute them are colibacillary, that is, the fibrinferment itself.

14. Diastases are not soluble ferments. Every diastase is a figurative ferment whose active element is bacterial in nature.

A) A diastase can only act on an indefinite quantity of matter if it is not subject to mass action; it is only removed from it if it multiplies; it can only multiply if it is constituted by a figurative and living element.

B) Any granulation of living matter smaller than one micron takes a spherical shape, is animated by Brownian motion, is bacterial in nature, and possesses fermentative power.

C) Any granulation of living matter animated by Brownian motion possesses a whole series of fermentative properties, not just one.

D) It is the nature of the substances to be transformed that determines the nature of the fermentative action.

E) The false principle of the morphological analogy trap is devoid of any meaning and is based only on hypotheses; a 0.5-micron granulation of living matter is, by this fact alone, a bacterial element regardless of the environment in which it is found, in vitro or in vivo. This false principle was only devised to support the false dogma of the asepsis of living organisms and to combat their bacterial nature.

15. The form of the original viruses of heterogeneous diseases is not that which has been attributed to them until now.

The Jennerian virus and the variola virus have been considered filtering and invisible viruses. However, the virus, in both cases, is a hyphomycete whose mycelium and innumerable conidia of 2 to 6 microns in size cannot be overlooked in the pustules.

The measles, scarlet fever, and chickenpox viruses are also hyphomycetes.

The original diphtheria virus is not the **Klebs-Loefler** bacillus; it is a hyphomycete of the *Cladosporium herbarum* form or another conidial form of cereals. The diphtheria bacillus, although capable of reproducing the disease, is only a secondary formation of the *Cladosporium* mycelium, which entirely constitutes the false diphtheria membrane. It is not the original infecting virus.

The same is certainly true for the viruses of post-mortem, typhoid fever, exanthematic typhus, poliomyelitis, etc.

For syphilis, the virus is not *Treponema pallidum*, which is only a spiral appendage of the mycelium of the hyphomycota that I isolated, cultivated, described, and photographed in the first volume of this work, and which constitutes the entire initial chancre: The mycelium of this hyphomycete bears an innumerable quantity of spiral appendages which are the spirillae (see fig. 4 and 6, pl. 283, 1st vol.) and which attests that this hyphomycete is indeed the syphilis virus.

The malaria virus is surely also a hyphomycete, or rather a group of hyphomycetes, because there are certainly several types of this virus. I isolated one of them from the blood of a patient in the middle of a malaria attack with a high temperature; this hypho-

mycete also developed spontaneously on the surface of the patient's blood, preserved at room temperature; it is a hyphomycete of the conidial form Spicaria of greenish yellow color. The patient's blood contained numerous oval conidia of this hyphomycete, and also spore-bearing hyphae.

The hyphomycete and its elements contained in the blood are photographed in plates 293 and 294 of the first volume of this work.

There can be no doubt that this hyphomycete was indeed the virus of this clear and characteristic case of malaria, the blood containing spore-bearing hyphae and spores exactly similar to those of the greenish-yellow hyphomycete spontaneously developed on the blood, some even still attached to large mycelial filaments vegetating in the blood.

The conidia contain a number of fine granules, which are microspores or zymo- spores, driven by Brownian motion. I think it was these zymospores that **Laverau** mistook for a hematozoan. By virtue of their Brownian motion, they can perforate the membrane of blood cells and infect them.

The fact that these microspores are formed by the conidia of a hyphomycete of the form *Spicaria*, a certain fact, attested by the abnormal elements contained in the blood, demon strates that the malaria virus is not a hematozoan, but a hyphomycete and, secondly, that the complicated evolutionary cycle imagined for the development of this hematozoan is inaccurate and purely imaginary.

Contamination by the bite of a mosquito that has just bitten a malaria patient can occur; but it does not show the nature and origin of the virus in the first malaria patient(s) at the beginning of the season when the mosquito hatches. It is probably by ingestion of a viral food that the infection occurs.

16. I will briefly recall only the considerable error that attributed to Koch's bacillus the quality of a heterogeneous virus when it is the dumbbell organite constructing anatomical elements and in particular a nuclear dumbbell with properties modified by degeneration.

17. Bacteria do not have cilia, as described.

The filaments they can emit are mycelial in nature and do not exhibit any movement. When they are motile, they are passively moved by the Brownian movement of a granulation that has attached itself to a filament.

18. Bacillary elements are immobile.

The movements exhibited by certain bacilli in a liquid are passive, determined by the Brownian movement of one or more granulations that attach themselves to them or to a fibrinous filament connecting a granulation to the bacillus.

19. The Pasteurian dogma of contagion is false.

This is one of the biggest errors in bacteriology. Diseases caused by heterogeneous viruses are not contracted through contact with an infected individual, but exclusively through ingestion of the virus, the original source of which is generally a plant food.

* * *

All these errors attest to the current backward state of bacteriology, over which the pasteurian school sought to maintain a monopoly and which, through the total absence of research and progress, has led to its current collapse, which is due mainly to respect for the four false pasteurian dogmas, conformism, and a tactic developed three-quarters of a century ago, by which the pasteurian school sought to stifle all new notions that were in opposition to the false pasteurian dogmas and likely to harm the sale of serums and vaccines.

CHAPTER VII

THE STRUGGLE OF THE PASTORIAN SCHOOL AGAINST THE TRUTH

HISTORY OF THE CONTINUOUS AND SYSTEMATIC OPPOSITION OF THE PASTORIAN SCHOOL AGAINST ALL THE NEW NOTIONS THAT HAVE DEMONSTRATED THE FALSENESS OF PASTORIAN DOGMAS FOR THREE-QUARTERS OF A CENTURY.

In the course of this study, we will examine the following points in turn:

I. Béchamp and the theory of microzymas.

II. Altmann's "Elementarorganismen."

III. The work of **Galippe**.

IV. Portier's symbiotes.

V. The strangulation of freedom of opinion, discussion, and scientific publication in France. The real motives that provoked it. Its harmful effect on the general interest and on the progress of science.

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1. BÉCHAMP AND THE THEORY OF MICROZYMAS

This chapter should begin with an account of the memorable discussions, or rather the stubborn struggle, that took place between **L. Pasteur** and **A. Béchamp** on the subject of fermentation and the phenomena of putrefaction. These discussions necessarily had to appear in the chapter on the control of pasteurian dogmas; they constitute the beginning of the pasteurian school's struggle against the truth. They will not be repeated here because they would be redundant.

What has become necessary in this chapter, however, is an analysis of **Béchamp's** work and a correction of some of his inaccurate conclusions.

Already in **Buffon's** time, the presence of granules called molecular or organic in the body's tissues was known. They were described by histologists (**Schwann, Henle**) in the first half of the 19th century.

Leeuwenhoek had demonstrated, as early as 1680, that brewer's yeast is made up of living globular elements, developed by vegetation, a fact confirmed and developed by **Cagniard-Latour,** then by **Turpin** and **Trecul.** In 1835, **Turpin** (80) described, under the

name of seminuliferous globulins, very fine granules which were formed by the globules or cells of brewer's yeast and which their bursting released; he recognized that the globulins, the active agents of the yeast, were capable of reproducing it completely.

Turpin noted the existence of granules identical to the globulins in different organic products; because of the ability of the globulins to reproduce yeast and to transform into *Penicilium*, he attributed to them a histogenic role in living organisms.

In various publications, **Béchamp** had reported, in sugar solutions in fermentation, and then in 1866 in chalk, the presence of granulations that he called "small bodies." It was only later that he noticed these granulations in the tissues of the organism where they were already known under the name of molecular granulations.

In 1867, **Hallier** (30) announced that he had observed in plants extremely small living granules, which are capable of multiplying and evolving by taking the form of chains, granulations, bacilli, filaments and of transforming into molds, into *Penicilium* for example, the latter being able, by regression, to return to the initial granular form to which the author gave the name micrococcus.

It is certain that these **Hallier** micrococcus were indeed the molecular granules, previously known and that they correspond exactly to **Béchamp's** microzymas. The latter challenged him in a discussion (Les microzymas, P. 611) which does not lead to conviction and from which he concluded: "that there is nothing in common between **HALLIER's** principles and the theory of the microzyma." What must be recognized is that, in March 1867, **Hallier** brought new knowledge on molecular granulations more extensive than that which **Béchamp** had made known at that time.

Later, **Bechamp** studied the role of microzymas in putrefaction and from that moment engaged in a fierce and very tough fight against the false dogmas elaborated by **Pasteur**. This is certainly the most important part of his work.

In his book, he discussed at length and claimed the discovery of the fermentative properties of his microzymas.

Fairness requires us to acknowledge that it was **Leeuwenhoek** who, as early as 1680, showed that a ferment such as brewer's yeast is made up of living globular elements that are born and develop by vegetation, and that it is to **Turpin** that we owe the knowledge, through the phenomenon of the bursting of the globular elements of yeast and the dissemination of the globulins it contains, that they are the active elements and that they are capable of reproducing it completely.

There is no doubt that these globulins are the microzymas of brewer's yeast since **Béchamp** himself attributed this nature to the granulations or "small bodies" whose presence he observed in sugary solutions where mold was growing.

The principle of the nature and living state of ferments, established since 1680, was therefore completed in 1835 by the morphological knowledge of the active element possessing the fermentative action: **Turpin's** globulin.

Since brewer's yeast does not differ essentially from the molds of which it is the vegetative form in a liquid medium (*Penicilium*, see 1st vol., p. 662), it follows from these facts that the discovery of the nature of alcoholic fermentation, or other fermentations by molds, is due neither to **Pasteur** nor to **Bèchamp**, but to **Leeuwenhoek, Cagniard-Latour, Turpin,** and **Trecul.**

The Microzyma Theory

Here is how **Béchamp** sets out the microzyma theory in his book (p. 564):

The theory of the microzyma results in a great unity. Microzymas are structured and living; they can multiply and communicate to the matter that serves for their multiplication, the property that is in them, the chemical and physiological activity that characterizes them, because they transform this matter into their own substance and it becomes what they are. In the organism, the cells, all the cells, are first the fruit of their activity, and these cells in their turn being constituted, I repeat, are apparatuses in which these microzymas acquire new aptitudes, by undergoing a sort of incubation there, while they multiply there: this is how the yolk microzymas become microzymas of the liver, microzymas of the pancreas, microzymas of the pepsin cells, nervous microzymas, microzymas which, at a given moment, will acquire the fertilizing property in the spermatozoon..., etc. These are not gratuitous assertions, but observed facts.

These are, on the contrary, either gratuitous assertions or false assertions, resulting in part from fundamental errors of observation by **Béchamp**. Let us examine them successively:

IS THE MICROZYMA STRUCTURED?

Béchamp defines it thus on page 275 of his book: "The microzyma is organized, structured, it is morphologically defined, to speak like Claude Bernard."

This is a gratuitous assertion. **Béchamp** has nowhere provided any indication of the organization of the microzyma. He only defines it, morphologically, as being a sphere of 0.003 to 0.0005 mm in diameter; in reality, its dimensions are on average 0.001 to 0.0002 mm.

The fact that the microzyma is a sphere with a precise outline is not enough to define it. The proof is that on page 600 of his book, **Béchamp** writes: "It is false that I considered all molecular granules to be microzymas." However, all are spheres exactly similar to microzymas, and **Béchamp** never indicated which ones are not; they all are in reality.

The organization of an element must be understood as an arrangement that demonstrates an arrangement suitable for an arrangement with other elements, because organization means arrangement, union, coaptation, or articulation of various elements together to constitute an apparatus. Now, the microzyma is spherical, not because of a structure, but because physical forces impose this shape on it as they do for a droplet of water and for all soft and plastic substances. The type of a structured element, already evolved with a view to organization, is the dumbbell organelle.

The microzyma, a free and mobile granulation, very different from the dumbbell organelle, is made up of two substances, one achromatic, the hyaloplasma, acidophilic, which constitutes its framework or substratum, and the other chromatic, either condensed into a small rounded mass if it occupies a central position, or in the shape of a cap if it has carried to the periphery of the element. This mass of chromatic substance is what **Enderlein,** in his nomenclature, designated by the word ***mich***, the entire granulation being for him the ***michit*.**

The fact that this small chromatic mass can be considered a nucleus does not imply an organization of the microzyma, nor a structure; it is a granulation of living matter, of unstructured appearance, the primordial bacterial element.

IN THE ORGANISM, ARE THE CELLS THE PRODUCT OF THE ACTIVITY OF THE MICROZYMAS, THAT IS, CONSTRUCTED BY THEM?

This is yet another gratuitous assertion resulting from **Béchamp's** technical errors. In every cell, he saw granules; he concluded that they were microzymas. This was an error. **Altmann** also saw granules in cells and, in addition, elongated rod-like elements; he concluded that the granules were indeed **Béchamp's** microzymas. He therefore made the same observational error as **Béchamp**, and here is why:

Béchamp indifferently called microzymas the free and mobile granules observed in blood, milk, and all organic liquids, and also those he observed in cells; he believed that they were an integral part of the latter's constitution, without indicating or knowing whether or not they retained their mobility and independence; in particular, he omitted to indicate what proved their quality as a constructive element of cells.

Now, in any well-fixed cell with well-preserved cytoplasm, we do not observe free granules, only the cytoplasmic network, with its dumbbells articulated together by their balls. The granules that **Béchamp** and **Altmann** saw in the cells are therefore the balls of the dumbbell organelles, whose existence escaped their observation, that is, the organelles that construct the cytoplasmic network in which they are totally immobile and rigorously fixed, a state that clearly distinguishes them from mobile microzymas endowed with Brownian motion.

The microzyma is a **fermentative, free, mobile element, endowed with Brownian motion,** while the dumbbell organelle, the builder of cells, is an **evolved, fixed element, definitively immobile** when it is articulated with others in the cytoplasmic and nuclear networks.

It is explained elsewhere that free and mobile colibacillary micrococcal granules from the blood do indeed penetrate the cytoplasmic cavity, but exit with the liquids secreted by the cell, urine, milk, digestive juice, etc., or with the lymph emanating from it; they only pass through it; they are not part of its constitution. In a well-fixed cell, we no longer see anything but the balls of the cytoplasmic dumbbells as granulations. We no longer see colibacillary cocci because, being free and mobile, having no connection with the cytoplasmic dumbbells, they are carried out of the cells by successive manipulations of the preparation, notably by washing, or even destroyed by the fixative.

Also, in an experiment like the one that follows, the colibacillary cocci or microzymas, are all carried out of the cells by washing.

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THE ALLEGED CONSTRUCTIVE PROPERTY OF MICROZYMAS, ACCORDING TO BÉCHAMP

Here, moreover, is the formal proof of the error made by **Béchamp** in asserting that microzymas are the building blocks of the body's cells and tissues. He provided this proof by studying the microzymas of the liver, pancreas, and blood cells. On page 228 of his book on microzymas, he writes that, once a cannula has been placed, pure or slightly creosoted distilled water is first passed into the liver through the vein, an operation he calls hydrotomization; then the following operations are carried out:

The liver washing being completed, all soluble matter being removed, it is reduced, by the as divided as possible. The pulp is placed in a very clean cloth knot, washed with potash, water, then boiling creosote water. Under a trickle of slightly creosote water, the knot containing the liver is kneaded with very clean hands, washed in advance with creosote water. The microzymas and unbroken cells pass through the mesh of the cloth. The cloudy liquid is left to stand; the free cells and nuclei settle first; in suspension and you see them in these

preparations, under the microscope; the microzymas remain longer; there are no more cells, no more nuclei. The supernatant liquid, which is cloudy, is decanted after ensuring again that the liver cells have been eliminated and is left to stand After 24 hours, enough of them have settled so that, despite their small size, they can be collected on a tightly woven filter. When the washing water no longer contains albuminoid matter, that is, no longer produces a precipitate upon addition of a volume three times that of highly concentrated alcohol, the washing can be considered complete.

Instead of proceeding in this way, it is preferable to pass the pulp, diluted in plenty of water, through a sieve, then through a fine, fairly tightly woven cloth; separation by decantation becomes faster.

After these long treatments, the microzymas were found unaltered; their shape and mobility remained the same.

Further on, on page 229, Béchamp adds: "I also separated the microzymas from the liver, not hydrotomized; they are apparently the same, at least morphologically."

Thus, the microzymas that **Béchamp** claims constitute the liver cell have a shape and mobility identical to ordinary microzymas, to those of the blood for example, and they are simple mobile granulations.

Let us first note that the two-hour washing with a stream of pressurized water drove the microzymas from the liver cells. Because of their mobility and Brownian motion, they were carried away with the water passing through the cells.

One might have believed, on the other hand, that **Béchamp** affirmed the constructive property of microzyma because he had established this proof by fixation and careful observation of well-preserved liver tissue. He did not perform this essential operation, and his observation is only that of easily alterable elements, successively subjected to multiple operations that profoundly alter them, operations such as a long stay in distilled water (at least 36 hours), washing the organ, washing with potash, then with boiling creosote water, and malaxation, operations which, in total, last at least 36 hours.

We can therefore affirm that the so-called microzymas observed by **Béchamp** under such conditions are only the debris of the dumbbells that constitute the cytoplasmic network of the liver cells, dumbbells of which only the balls that have detached from their rod remain. Plates 17, 18, 19, and 21 of this third volume, which so clearly show the constitution of the liver cell and its cytoplasmic network, formally confirm this conclusion.

The same error was made by **Béchamp** regarding the so-called microzymas that supposedly constitute the pancreas, the thymus, the spleen, the kidney, the stomach, and the blood cells. For these, he writes (Microzymas, p. 244) that their microzymas can be directly seen by a few manipulative devices such as this:

The blood is mixed with three or four times its volume of a saturated sodium sulfate solution and poured onto a filter. The globules are retained: they are detached and crushed with a glass wheel on a slide of the same substance: the globules are torn apart, and the microzymas, having become free, swim in the liquid with their own oscillatory movement.

Here again, it is only the balls detached from the dumbbells of the globular network that **Béchamp** observed. I remind you that this network is easily seen on the living blood cell by placing a drop of blood in 7.5% salt water strongly tinted with methylene blue. The network fixes the blue and thus appears very visible in its normal state. It is photographed thus in Figure 1, plate 37 ter of this volume, a reproduction of Figure 4, plate 108 of the first volume.

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However, despite his error, **Béchamp** points out that "while it is easy to see the microzymas of blood, it is very difficult to isolate and study those of blood cells separately, either because water alters or deforms them."

He also observes that the microzymas of blood cells are among those that produce bacteria with difficulty. This interesting fact means that, unlike the colibacillary cocci of the blood, the dumbbell organelle gives rise to a bacterial culture with difficulty.

It seems that these last two observations must have cast doubt in **Béchamp's** mind on the quality of globular microzymas. He nevertheless continued to falsely assert the constructive quality of the microzyma for the entire organism.

* * *

Thus, the assertion of the histogenic properties of microzymas is purely gratuitous; it was the consequence of the defective observations that I have just presented; one would search in vain, in **Béchamp's** book, for the slightest observation establishing this histogenic role.

On the other hand, I have established this role formally, materially, with regard only and exclusively to the filamentous elements of connective tissue. I have demonstrated and proven by photographs (fig. 4, pl. 37 ter and p. 219) that the colibacillary granules of the blood plasma germinate by emitting a long filament which, grouped with others, constitutes the bundles of fibers of the connective tissue.

This origin of connective fiber bundles, coming from large germinal masses of granulations, had already been indicated in the first volume of this work. As for the cellular elements of organs, the various elements of muscle tissue and nervous tissue, cells and neurofibrils, they are all constructed by the dumbbell. It remains to examine where this constructive dumbbell comes from. This question has already been addressed in the study of the histogenic role of the organic colibacillus.

ORIGIN OF THE DUMBBELL ORGANELLE

Does the dumbbell originate from the colibacillary cocci, which would emit a short rod and form a ball at its end? It is possible, but no direct observation so far allows us to confirm such a transformation.

It is probable because a micrococcus culture can pass into the bacillary, then mycelial, and finally *Mucor* and *Aspergillus* states, and the filaments of these are made up of dumbbells which, consequently, ultimately originate from the first micrococci.

But it is also possible that the dumbbell is transmitted directly with its shape into the ovum and the spermatozoon, and consequently into the egg, without passing through the colibacillary granulation state. What suggests this possibility is that:

1. When the dumbbell degenerates in animals, it becomes tubercular, but keeps the dumbbell shape and multiplies while keeping this shape;

2. In somatic kinesis, the network of nuclear dumbbells divides into network fragments that remain in the dumbbell state in the chromosomes, without returning to the granulation state, and are found in the same state in the two new nuclei. The same is true in meiotic division in plants, but in this case, the network of nuclear dumbbells disintegrates, in heterotypic prophase, into dumbbell filaments that segment and remain in the dumbbell state in the gemini.

Unaware that his observation of the constructing cellular microzymas was false, **Béchamp** extended his conclusions to the egg and made this new gratuitous assertion, which nothing serious justified in his observations, as we will see later, that it is the microzyma that makes the egg. We see from the above how risky such a conclusion is.

We can conclude from this presentation that nothing in **Béchamp's** observations authorized him to conclude that microzyma constructs cells and tissues, and he has provided no facts that support such a hypothesis and explain how this construction would take place.

In the exposition of the microzyma theory reproduced above, there is not a single line that does not contain an assertion exaggerating, even inaccurately, the scope of the facts that **Béchamp** has established. Nowhere has he demonstrated that microzymas are structured, that they undergo a sort of incubation in cells and acquire new properties there, that there are special microzymas in the liver, the pancreas, the nervous tissue, that they acquire the fertilizing property in the spermatozoon... etc. All these conclusions are gratuitous.

This entire theory boils down to confirming the existence of molecular granules already known, their state as living matter already known in brewer's yeast and **Turpin's** globulins, and their fermentative property already demonstrated by Turpin for these globulins. But **Béchamp** deserves the great credit for having clearly highlighted the cause of fermentation or putrefaction of organic matter, and the falsity of **Pasteur's** dogmas.

THE BACTERIAL NATURE OF MICROZYMA

In his book *Les microzymas*, **Béchamp** wrote (p. 147):

"*There is great interest in knowing whether or not bacteria can be born in animal tissues without the contribution of external germs. You are aware, moreover, that this is the point of the great debate between Mr.* ***Pasteur*** *and me.*"

If **Béchamp** wrote: "*if bacteria can be born in animal tissues...*", it is because he did not consider microzymas to be bacteria or microbes, while demonstrating that they can transform into them.

Nencki and **Giacosa** having written, in a documentary article, that **Béchamp** *"had admitted the existence, in all organs, of micrococcus that he called microzymas,"* the latter replied in his book (p. 155), in reporting this article, *"all this is very accurate, except for a few details of interpretation, the confusion of microzyma with micrococcus and the use of the word microbe."* On pages 149 and 150, regarding experiments on the liver, **Béchamp** wrote several times: *"Only normal microzymas are discovered there, not a bacterium."* Further on, he specifies: *"There are many granulations associated in a string; there are no bacteria yet."* He therefore did not even consider the granulations associated in a string (streptococcus) as bacteria.

Béchamp therefore had a singular conception of the nature of microzymas. It is apparent in his work that he denied them the quality of bacteria because he judged that by recognizing this quality, he would be classified in the sect of sponteparists, which he vigorously defended, having, he said, carried out experiments against spontaneous generation. It is also clear that, unable to recognize this quality in microzymas, in which

he nevertheless recognized the power to multiply, he circumvented the difficulty by calling them *the previous state of bacteria*.

He only considered the bacterial state in the bacillary form since he did not consider the microzyma associated in a chain, which is the streptococcus, to be a bacterium. For him, the granulation remained a microzyma as long as it was spherical or slightly oval. As soon as it had the elongated shape of a bacillus, it became a bacterium.

I do not need to demonstrate again the inaccuracy of such a conception, which is established by the identification of the fibrinferment with the colibacillus, set out in the chapters relating to blood coagulation and the colibacillus function. Indeed, the microzymas of the blood are the serozyme or fibrinferment, and I have demonstrated that the active element of the latter is the micrococcal granulation of the organic colibacillus, the existence of which in the blood I have shown in the forms of cocci, bacilli, mycelial filaments, hematoblasts, and leukocytes (various germinative masses).

This identification of fibrinferment with colibacilli, the mechanism and role of the phenomenon of blood coagulation, the role of colibacillus cocci in the blood in the generation of digestive ferments, the original source of bacteria in the digestive tract and the mouth, definitively establish both: the bacterial nature of the animal organism, the

colibacillary function of higher animals (bacterial function of living beings, animals and plants) and, naturally, the bacterial nature of **Béchamp's** microzymas.

This bacterial nature was, moreover, evident, against the latter's opinion, because he declared microzymas to be the previous state of bacteria, and because on page 742 of his book, he concluded: "*Bladder microzymas, like all microzymas, can evolve and become bacteria; but these bacteria, by regression, can reproduce microzymas*."

These two statements constituted proof that granulation and bacillus are two different forms of the same evolving bacterium. In reality, these conclusions, like a host of others by **Béchamp**, are purely imaginary; they are only proof of incomplete, insufficient observations, and of the necessity he found himself having to accommodate his interpretations with religious conceptions. The only complete observation of blood would have shown him that the bacillary form, which he considered alone to be bacterial, always exists normally alongside the granular microzyma form; that the blood bacilli contain within themselves these same granulations which are their spores, and that they themselves segment into small elements which become granular. In summary, **Béchamp** drew false conclusions about the nature of his microzymas. They are bacteria and the cocci form cultures of the colibacillus.

It is therefore not only microzymas that the animal organism contains: it is a complete bacterial culture with micrococci, bacillary and filamentous elements, and germinal masses, which are the mononuclear and polynuclear leukocytes.

On the other hand, **Béchamp** did not see that, in the normal organism, the multiplication of cocci, which are microzymas, occurs mainly by a short filament emitting fine spores, while, outside the organism, in the phenomenon of putrefaction, it is mainly bacillary forms that develop by germination of cocci grouped into germinal masses.

EXAGGERATIONS OF THE MICROZYMA THEORY

Béchamp's book: *Les microzymas*, although very remarkable, is subject to serious criticism. This book is the publication of the text of the lectures he gave to his students at the Free University of Lille. These lectures are not written in the succinct and precise manner used in a memoir intended for a learned society. **Béchamp** gave free rein to his imagination and to developments that often con-siderably exceed the scope of the facts he established through his experiments. He often allowed himself to be led to purely imaginary conclusions that require rectification. Here are a few examples, which I will follow, each time, with the necessary criticism:

... Page 127: The theory of protoplasm is erroneous in its current conception as well as incomplete: there is nothing living in it but the microzyma, just as in a blastema.

This statement is an error and has no meaning, having been made by **Béchamp** in his total ignorance of cellular structure, the existence, role, and physiological constitution of the dumbbell organelle, and based, on the other hand, on an inaccurate fact: the microzyma, an **essentially mobile element,** is without contact relations with any figurative element, and cannot be a constituent part of the protoplasm, which, as we know, is the cytoplasmic network exclusively constituted by dumbbell organelles articulated together and **strictly immobile.**

... Page 535: The role of the cell is secondary; the cell exists only through the microzyma, is governed only by the microzyma...

This is an imaginary and inaccurate conclusion that is not supported by any of the facts established by **Béchamp**, the organelle that constructs the cell being the dumbbell organelle and not the microzyma.

Page 537... The cell is an important element of organization. But, once again, it is a transitory organism which does not fulfill the conditions of the essential, autonomous organized element, having life in the soil by primitive destination, which philosophy seeks. I repeat, this essential element is microzyma.

It is also a conclusion which results only from reasoning and not from material facts; it goes beyond the significance of the facts established by **Béchamp.**

It is false that the cell is a transient organized element. It is, on the contrary, a stable, fixed, indispensable element; it is the basis of organization and the basis of the vital phenomena which result from it; does the neuron therefore not fulfill the conditions of essential organized elements?

Without the cell, without the already organized dumbbell organelle, there can be no muscle, no gland, no blood circulation, no sensitivity, no vision, no hearing, no voluntary movement.

For his postulate to be correct, **Béchamp** would have had to previously prove that the colibacillary cocci free from the blood is capable of completely reconstituting, at any time and indifferently, both a liver cell and a pyramidal cell of the cerebral cortex.

Much more than the colibacillary cocci or microzyma, the dumbbell organelle could be considered as the essential organized element because, constituting the totality of the tissues of all beings in the two kingdoms, it already bears the mark of an obvious organization which designates it for its function.

* * *

In summary, **Béchamp** put forward a large number of ideas about the microzyma, and thereby demonstrated a very fertile imagination, but many of them are errors. We should only retain those that are supported by accurate material evidence.

He clearly saw the fermentative role of molecular granules, their intra-organic origin, and their power to evolve into bacterial elements, a fact which, however, appears to have been seen before him by **Hallier**, who even recognized the possibility for them to transform into hyphomycetes, which, in turn, can return to the bacterial state.

He had the great merit of providing new demonstrations of already known facts, of consolidating them, of making their significance better known; he had the merit of being the first to establish the cause of putrefaction, diametrically opposed to **Pasteur's** theory. He shares with **J.-B. Dumas** and **Frémy** the considerable merit of having energetically and tirelessly fought against **Pasteur's** false dogmas of atmospheric panspermia and the asepsis of living beings.

It is known that **Pasteur** could not tolerate or admit contradiction, and that he manifested a keen irritation that often led him to respond in a way that was hurtful to his opponents. **Frémy** had to endure attacks of this kind. But it was above all **Béchamp** who had to sustain, for many years, a violent and very harsh struggle against **Pasteur.**

Professor **Rappin**, director of the Pasteur Institute in Nantes, spoke out in his interesting work on *The Etiology of Infectious Diseases* (71, p. 19) "*against **Pasteur's** attitude towards **Béchamp**, which was sometimes unacceptable, particularly with regard to the incident that occurred in 1881 in **London.***"

Here is how **Béchamp** relates this incident in his book *Les microzymas* (p. VII):

*In a sectional session of the International Medical Congress in London in 1881, the agenda called for a discussion on the role of bacteria in diseases. **Mr. Pasteur** took the floor, and suddenly, while I was present, before I had said a word, he first confused me, in a common anathema, with all the followers of heterogeneity. I let him speak because I was supposed to speak after him. But soon, I was obliged to come down from my seat and sit opposite Mr. **Pasteur** because he had dared to say: "That if there was something correct in my way of seeing, I had only conceived it by assimilating his works and modifying my ideas according to his." In short, **Mr. Pasteur** had just formulated a general claim of priority, and the most unheard-of accusation of plagiarism. In an indignant voice, I immediately challenged Mr. **Pasteur** to prove his assertion, warning him that I would prove the opposite to be true. **Mr. Pasteur,** refusing a public discussion, left the session. The Times of August 8, 1881, recorded the incident.*

Let us note here that it was a misinterpretation to assert that **Béchamp** had been inspired by **Pasteur's** ideas and work, since his conclusions are exactly the opposite and demonstrate the falsity of **Pasteur's** dogmas, in particular all his conclusions on the putrefaction of corpses, meat, blood, milk, and urine, as well as those relating to atmospheric panspermia.

Such assertions, so contrary to the truth that one does not even dare to support them in public, show the nature of the methods of discussion employed against **Béchamp**, and make one understand the righteous indignation that the accusation of plagiarism provoked in him.

But this struggle now enters the domain of history. No one will prevent what is rightfully due to **Béchamp** from being given back to him, and the false opinions that have been spread about his work from being corrected. The publications of the two contradictors remain with their dates to allow for an impartial judgment and the fair asses - tment of what is due to the discoveries of those who preceded them.

* * *

It follows from the discussions just presented that **Béchamp** clearly saw and demonstrated the falsity of pasteur's dogmas; but intensive scholarly propaganda and smear cam-

paigns undertaken on all sides against his work have not allowed him to make his conclusions prevail.

It is error that has triumphed: the false pasteurian dogmas have erected an insurmountable wall in the face of scientific progress and, through the false direction they have given to research, are responsible not only for halting the progress of science but also for numerous new errors that have been added to the original ones.

By stubbornly supporting, with indomitable courage, a hard and unjust struggle to defend the truth against false and harmful dogmas, **Béchamp** has deserved to be remembered with great admiration for the elevation of his character; through the great importance of the progress he has brought to science, he has left the memory of a high intelligence and a high scientific value that ranks him among the most eminent scientists of the 19th century.

The triumph of false pasteurian dogmas and the oblivion into which **Béchamp's** great discoveries on microzymas, fermentations, and putrefaction have fallen are the greatest scientific injustice of the 19th century and, at the same time, the most harmful scientific event of that century for the progress of science.

II. ALTMANN'S ELEMENTARY ORGANISMS

Since 1880, and given the great danger that **Béchamp's** new notions created for pasteurian dogmas, they were undermined on every possible occasion, minimized, and declared without interest.

In 1890, a German scientist, **Altmann** (1), announced that he had discovered, in tissues, elements that he called "*Elementary organisms*." These observations were, morphologically only, a little more complete than **Béchamp's** because they revealed that, while some of these elementary organisms have the shape of granules similar to that of microzymas, others have the shape of rods or filaments.

In the theory he established on the role of these elementary organisms, **Altmann** criticized **Béchamp's** conclusions and wrote:

*When **Béchamp**, misled by a faulty observation of the processes of putrefaction, admits a direct evolution of the elements of the cell into independent organisms, and thus puts identity in the place of analogy, this is in contradiction with everything we have learned so far through accurate observation of organized matter.*

Altmann has, by this sentence, expressed an erroneous opinion about **Béchamp's** work on putrefaction; in the first place, it is not the elements of the cell that **Béchamp** has not

admitted, but formally demonstrated a direct evolution into bacterial elements, but only the microzymas of blood, urine, and milk.

In the second place, **Béchamp's** observations on the processes of putrefaction are not faulty, but perfectly exact, free from criticism, and very demonstrative by a series of varied facts.

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Not only has he not put identity in the place of analogy, but he has made too much distinction between these two things by considering microzymas as bacteria or microbes when they really and exactly have this nature, being the micrococcal elements of the organic colibacillus and the elements classified by bacteriologists as streptococcus, staphylococcus, pneumococcus. The demonstrations made in this book relative to the colibacillary function provide proof of this.

It is therefore **Altmann's** opinion that has been distorted because, influenced by the false pasteurian dogmas, he wanted to bring it into line with them without first verifying its accuracy. It is he who, on the contrary, committed an error by making a distinction between identity and analogy when it comes to comparing the nature of granular bacteria (cocci) and granulations of free living matter of the same size; the latter, by the sole facts of their size, their shape, their quality as living matter, which give them the property of multiplying and the fermentative power, are bacteria and cannot be distinguished from the latter by any characteristic.

Indeed, the fermentative property is inherent in living matter by its very nature, since it can only live by assimilating the nutrients of the environment in which it lives, and it is necessarily necessary for it to transform them to make them assimilable, an operation which is the fact of all fermentation.

Ferments derived from animals and plants are active through the granulations which constitute them and which have the quality of bacteria which are easily transformed, as I have done, into elements of bacillary form.

In this matter, the new notion of the crucial colibacillary function of animals provides precise proof and definitively condemns the false pasteurian dogmas.

What **Altmann's** imprecise assertion that **Béchamp's** observations "*are in contradiction with everything we have learned so far by exact observation of organized matter*"certainly means is that they are in contradiction with the false dogmas of atmospheric panspermia and the asepsis of living organisms: on this point, there is evidence; but to affirm that these dogmas result from exact observation of organized matter proves that **Altmann** admitted the accuracy of pastorian dogmas without verifying them and that he was ignorant of the research of **Leeuwenhæk** (1680), **Cagniard-Latour**, **Turpin**, **Trecul**,

Frémy, and especially that of **Hallier** (1867). **Béchamp's** work is in perfect agreement with that of these scientists, so one must consider imprudent and premature the following approval of **Altmann's** assessment that **Nageotte** formulated as follows in his book on the organization of matter (52).

This assessment, whatever one's idea of the genesis of the cell, and whatever the opinion adopted concerning the analogy that exists between microorganisms and mitochondria, has lost none of its value since it was admitted. It can be applied to all the attempts that have been renewed on different occasions and in different forms to revive **Béchamp's** illusions.

A few lines above, **Nageotte** wrote, concerning **Altmann**:

*While considering his elementarorganisms as the objective form of **Béchamp's** microzymas, he strongly opposed the opinions of this author who, with the well-known obstinacy, believed he could directly cultivate microzymas, that is, ultimately, granules.*

This paragraph contains two errors, one by **Altmann**, the other by **Nageotte.** By considering his elementarorganisms as the objective form of **Béchamp's** microzymas, **Altmann** made a mistake, because, in cellular organization, there are no microzymas, essentially mobile elements. It is also an error by **Béchamp** to have attributed the construction of cells and tissues to microzymas. It is the dumbbell organelle alone, unknown to **Altmann** and **Béchamp**, which possesses this property.

By claiming that **Béchamp** believed he could cultivate microzymas directly, **Nageotte**, for his part, made another error, because **Béchamp** formally demonstrated that the microzymas of blood, milk, and urine evolve by giving rise to bacterial forms, chains of cocci and bacilli.

These reflections only prove that **Altmann** and **Nageotte** accepted pasteurian dogmas without having checked them and that they criticized **Béchamp's** observations without having verified them. If they had carried out this check and verification, as I myself did, their opinion would inevitably have been that pasteurian dogmas are false and **Béchamp's** observations are correct.

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Neither **Béchamp** nor **Altmann** saw the shape and constitution of the cell-building dumbbell organelle, nor its mode of articulation, nor the constitution of cellular organization by the networks of cytoplasmic and nuclear dumbbells. The elements described by **Altmann** are, on the one hand, isolated granules, and on the other, distinct filaments, without any indication of a connection or any relationship between them. The elements described by **Béchamp** in the normal state in cells are exclusively granules. However, it

has been demonstrated above that **Béchamp's** observations concerning the property that microzymas would have of building cells and organs are inaccurate.

Finally, let us note that it was **Altmann's** research and very incomplete results that prompted subsequent research on mitochondria, notably that of **Regaud** and **Guilliermond**. **Nageotte** wrote in his book, page 109: ***Altmann's*** *morphological description is impeccable, and nothing essential has been added.*

Then, page 111:

The anatomical basis of his theory cannot be attacked, and recent advances in plant cytology, due mainly to the work of ***Cowdry*** *and* ***Guilliermond****, have proven the accuracy of his descriptions, by showing that granula really exist, and that they have the same shape in the living state as in the preparations where the histological technique had allowed them to be discovered.*

The reader who has read the first chapters of this third volume and noted that the existence of mitochondria results only from a long series of gross errors of observation; that the elements described under this name are only the scattered and shapeless debris of the dislocated cytoplasmic network and the dumbbell organelles that constitute it, destroyed by the fixative, will certainly judge that **Nageotte's** conclusions are at least premature and that before making such assertions, it is always prudent to check them.

III. GALIPPE'S WORK

Galippe, who published two notes on the presence of microorganisms in plant tissues, is one of the most striking examples of the influence of pasteurian dogmas on the direction of scientific research. At the beginning of his second note of October 15, 1887, he writes, regarding the objections made to him regarding a previous note:

... I will not dwell on the purely theoretical objections made to me. If, on the one hand, my experiments are in opposition to the ideas currently accepted in science, I do not attach any importance to them, since respect for acquired or reputedly accepted notions would be an obstacle to the very progress of science.

Galippe (23) demonstrated, in impeccable experiments, the formation of bacterial cultures by fragments of tissue from various plants extracted aseptically (from the central part of a carrot, for example) and placed in various culture media.

Although an independent, nonconformist spirit, as his opinion reported above proves, **Galippe** did not seek to realize the determinism of the very important fact he had just observed, and which clearly invalidated, for plants, the pasteurian dogma of the asepsis of living organisms. But, convinced by this dogma that the nature of the living matter of

organized beings has nothing in common with the nature of the bacterial living manner, he explained the fact by concluding:

1. That the microorganisms contained in the soil can penetrate plants;

2. That plant tissues do not offer effective resistance to the penetration of microorganisms.

He thus provided proof of the false orientation that pasteurian dogmas had given to his research, despite his independence of mind.

* * *

IV. PORTIER'S SYMBIOTS

Through direct microscopic examination of the organs of certain animals, insects, and invertebrates, **P. Portier** directly observed the presence of microorganisms. Aseptically extracting a fragment of fatty tissue or certain organs such as testes, ovaries, muscles, etc., which he transported to various environments, he obtained bacterial cultures. He described the microorganisms in these cultures as follows (62):

Their form is extremely variable. The same microorganism can successively, and depending on the culture medium, appear as a micrococci, a bacillus, or filaments reaching great lengths. This is a statement that will arouse suspicion in the minds of bacteriologists, who will wonder if my cultures are pure. They are, without any possible doubt; this is a point on which there will certainly never be any discussion.

P. Portier considered these microorganisms as symbiotic bacteria which, penetrating the internal environment through the intestinal tract, would have the role of renewing the mitochondria.

What **Portier** discovered again was exactly what **Béchamp** had already discovered with his microzymas, and which he had defended so energetically against **Pasteur's** violent and constantly repeated attacks. It was also what **Galippe** had discovered in plant organisms, and which his belief in pasteurian dogmas had prevented him from accurately appreciating. Therefore, he soon came under attack from the pasteurian school. He was attacked by **Guilliermond** and **Cl. Regaud** (68).

Regaud examined this precise point in his critique: are mitochondria microbes? He then examined the properties of mitochondria and those of microbes to identify fundamental differences. He wrote:

Mitochondria have a definitively established objective existence... The work of recent years has legitimately elevated them to the rank of fundamental organelles of the cell... They have an analogy of form with microbes that all observers have noticed but which is not constant..., etc.

And he concludes:

Ultimately, apart from the similarities in form, the exceptional coincidence of a similar colorability, and the common power of chemical synthesis that belongs to all living matter, ***there are only divergences between mitochondria and microbes****... If Mr.* ***Portier*** *admits this, he will still have to demonstrate that these two objects transform into each other.*

Taking up **Altmann's** erroneous objection, **Regaud** adds:

Ultimately, I think that ***Portier****, falling victim to the trap of morphological analogies, confused mitochondria and microbes. Taking up* ***Altmann's*** *theory of "bioblasts" (who was the true author of the discovery of mitochondria, whose granules and filaments he called "elementary organisms"), Mr.* ***Portier*** *extended it and, I would dare say, aggravated it.*

However, he wrote: "Nothing is more misleading than a morphological appearance. The shape of mitochondria alone cannot authorize us to conclude that they are microorganisms."

Based on these essential rules of scientific interpretation, I am obliged to conclude that our colleague of protoplasmic structures seems to me to have transgressed them without knowing it and perhaps because he had not sufficiently experienced the technique.

Portier immediately responded to these criticisms with the following very just objection:

... Any discussion on the subject at hand should be based on a precise definition of mitochondria. Only this definition would make it possible to identify or differentiate any intracytoplasmic formation from a mitochondria.

However, I find this definition neither in the classical treatises nor in the original memoirs that deal with the question. It seems to me very difficult to give.

It cannot be a morphological definition, since ***Regaud*** *himself has just reminded us of the polymorphism of these organelles, which he also emphasized in many of his works.*

Regaud, responding to this observation by **Portier**, completed the definition of mitochondria as follows:

Mitochondria are not a vague concept of histologists. They are precise objects, which can be easily defined according to all of their properties: corpuscles of shapes exactly circumscribed by a clear outline, most often spherules or filaments; of fluid consistency; suspended in the cytoplasm, where they occupy a situation sometimes arbitrary, sometimes characteristic of the cellular species or functional phase considered.

I limit this quotation to these different characteristics, which are more than sufficient to justify the following assessments:

Regaud describes as precise objects defined as being, morphologically, *''Corpuscles of shapes exactly circumscribed by a clear outline, most often spherules or filaments*."

Let us add to this that these very precise objects have such imprecise shapes that, by searching in the figures of **Guilliermond's** memoir, analyzed at the beginning of this volume, we find that these disparate objects present more than fifty different shapes, of which a figure from a memoir by **Milovidov** (50) shows a certain number well aligned. I have reproduced this alignment in plate 15, figures 5 and 6 of this book in order to properly inform the reader.

It is this diversity of disparate, shapeless, and scattered objects that should be called the trap of morphological diversity, a trap into which those who have studied mitochondria have fallen; in reality, it is the trap of the fixing reagents, called mitochondrial, which, contrary to the assertions of **Regaud** and **Guilliermond**, preserve mitochondria so poorly that they destroy, dislocate, and render unrecognizable the real objects from which they come.

These supposed characteristics of mitochondria, which remain suspended in the cytoplasm and occupy a situation that is sometimes random and sometimes characteristic of the functional phase considered in **Regaud's** definition, are, on the contrary, proof that they are only the debris of destroyed constructive organelles.

It is demonstrated at the beginning of this book that mitochondria have no real existence and that the so-called precise objects described under this name are only the shapeless, scattered debris of the dumbbell organelle and the cytoplasmic network it forms, the universal constructor organelle of the cells and tissues of animals and plants.

This organelle is precise, already differentiated into the two parts that enable its constructive role: the two balls by which it articulates with other organelles and the rod which, connecting these two balls, constitutes the resistant bar that ensures the solidity as well as the elasticity of the dumbbell network.

This organelle is not semi-liquid as **Regaud** assures; it is resistant and to such a point that the myelin sheath it forms (see page 79) has been called the neurokeratin network and it was by subjecting the nerves to the digestive action of pancreatic juice, which it resists, that **Ewald** and **Kühne** (19) discovered it.

The universal dumbbell organelle has only one form, rigorously constant; its size alone varies in large proportions; its morphological definition is precise:

it is a straight or slightly arched rod, bearing at each of its ends a spherical granulation with a diameter double, triple or quadruple its own, basophilic when it is loaded with chromatic matter, acidophilic when this has disappeared, strongly stainable by the FEULGEN reaction when it is a nuclear dumbbell loaded with chromatin, and barely or only slightly if it is a cytoplasmic dumbbell.

This is precisely the definition that **Portier** was calling for.

But this definition, or rather the dumbbell shape of the elementary and universal building organelle, completely transforms the aspect of the question. Indeed, it is not the dumbbell organelle that **Portier** observed in tissues, but mainly granules. He described, when it comes to the shape of bacteria in cultures:

Their form is extremely variable. The same microorganism can successively and depending on the culture media, appear as a micrococci, a bacillus, or filaments reaching great lengths.

This proves that, as in the case of **Béchamp's** microzymas studied previously, **Portier** did not sufficiently specify the quality and origin of the objects he observed in the direct examination of the tissues. It was not the haltere organelles that gave rise to the cultures he obtained, but granules which are the equivalent of the **Béchamp's** microzymas and, in nature, the granules of the organic colibacillus, active agents of the colibacillary function.

As I demonstrated above, these granules have nothing in common with the haltere organelle and consequently with the mitochondria, which are only fragments and shapeless debris of the latter and of the cytoplasmic network that it constitutes. As for the bacillary and filamentous forms of the cultures, they are the transformation forms of the colibacillary granules, transformations well explained in this volume and already in the first volume of this work.

The objects observed directly by **Portier** therefore have no contact with the mitochondria, and the discussions which took place on this subject could not lead to any result:

1. Because, on both sides, they were discussing two totally different objects;

2. Because one of the two objects, the mitochondria, has no real existence, and properties have been attributed to them that they do not have;

3. Because the real object that alone should have been the subject of the discussion, the dumbbell organelle, was still unknown;

4. Because the objects observed by **Portier** were the elements of the organic colibacillus (fibrinferment), that is, the active elements of the colibacillary function, elements whose quality was still unknown.

How, in these conditions of obscurity, could such a discussion have led to a satisfactory result? This is an example which shows that, in the still backward state in which general biology, and especially bacteriology, finds itself, it is appropriate to approach such discussions only with the greatest caution.

* * *

At the request of the Société de Biologie, Messrs. **L. Martin** and **Marchoux**, of the Pasteur Institute, were appointed to carry out control experiments, in conjunction with Messrs. **Portier** and **H. Bierry**. These resulted in the following conclusions (Soc. de Biol. 1919):

1. *Transporting pieces of an animal's organs into culture media is always difficult to achieve with constant asepsis. It is one of the most delicate operations in bacteriology.*

2. *Cultures are generally not obtained from healthy organs when, to inoculate the media, testicular pulp collected using a tapered* ***Pasteur*** *tube is used.*

3. *Microbes may be found in the testicles, in proportions that would require a considerable number of experiments to determine, when operating with whole organs or large fragments. The presence of these microbes in the testicles is not a constant fact; it is impossible to affirm their existence in the normal state.*

It seems that **Portier** himself acknowledged that these conclusions were well-founded. I believe that the conclusions of this report are inaccurate statements, devoid of any precision, and that, on the contrary, they demonstrate the accuracy of the results of **Portier's** experiments. Let us examine these three conclusions carefully.

FIRST CONCLUSION. - I believe that it is taking scientists for very clumsy and unintelligent people to claim that it is difficult to carry out the aseptic transport of a piece of organ in a culture medium, an operation that a well-educated laboratory worker succeeds in ten times.

This objection is false and worthless. To demonstrate it is false, it is sufficient to sterilize a testicle or a piece of liver at 120 or 130°C and perform 25 inoculations with them in broth or on agar with the usual precautions. The 25 tests will remain sterile. If I affirm this, it is because I have done it.

This first conclusion is the convenient and usual cliché imagined to cast suspicion on results opposed to pasteurian dogmas. Let those who doubt verify this for themselves and they will judge.

Moreover, the same objection, which is indeed the usual cliché, had already been made to **Galippe** in 1887, when he demonstrated that the culture of fragments aseptically extracted from healthy plants, such as carrots, turnips, radishes, celeriac, cabbages, etc., gives rise to bacterial cultures, and this with 100% success in certain series; he had responded to this objection with the following experiment that I relate verbatim (23, p. 559):

To determine the proportion of causes of error introduced into this type of experiment, I inoculated a certain number of tubes containing the culture liquids I usually use with inorganic bodies (pumice stone) sterilized by heat.

My experiment involved 79 tubes. I introduced several fragments of pumice stone into a large number of tubes. As a result, I had to open the bottle containing these fragments more than 160 times during the course of my experiment. This was carried out on July 19. To date (October 15), no tube has proved fertile. I am well aware that, in the inoculation of plant fragments, the causes of error are a little more considerable because the manipulations are longer. Nevertheless, I am convinced that by operating very carefully, the causes of error inherent in any similar experiment should be reduced to a negligible proportion.

After such demonstrations, the question is settled, and any sensible experimenter will consider that one must be very poor in grounds for serious criticism to formulate such objections, which do not even have the slightest appearance of plausibility and are only vain attempts to rescue pasteurian dogmas.

SECOND CONCLUSION. - I also believe that the second conclusion of the report was not written as it should have been. Why, instead of saying: **"We generally do not obtain a culture,"** meaning that a certain number of tests were carried out, not specify exactly: "in x cases out of y tests, a positive or negative result was obtained"?

Why, on the other hand, was the sampling operation performed with a tapered **Pasteur** glass tube, which, being very fragile, does not allow the organ to be shredded before aspiration, instead of using a sterile metal trocar specially mounted to allow this preli-

minary shredding, an operation which is essential for collecting a sufficient quantity of well-pulped substance? Done in this way, the operation gives 100% positive results.

With a glass pipette, only a little lymph is collected, very often too poor in formed elements to give rise to a bacterial culture. To obtain a culture, it is first necessary to carefully pulp the center of the organ with a special instrument, then remove enough pulp with a pipette. It is the absence of this precaution that results in the negative result of certain inoculations.

Despite the very poor procedure used, it remains nonetheless that, even in this case, there were positive results and that these are sufficient to affirm the accuracy of **Portier's** results.

This second conclusion of the report means only that the control proved powerless to reliably detect, in an organ that always and normally contains them by the billions, bacteria that are the micrococcal form of the organic colibacillus.

* * *

THIRD CONCLUSION. - Who will believe that a considerable number of experiments is required to establish the proportion of cases in which microbes are encountered in testicles placed in a culture medium when operating with whole organs? 20 or even 10 experiments were sufficient to lead to a decisive conclusion. But, for this to be valid, it is not only in the culture medium that the bacteria must be sought, but also in the center of the testicle or organ.

I formally dispute that the presence of these microbes in the testicles is not a constant fact and that it is impossible to affirm their existence in the normal state. I formally dispute the accuracy of this conclusion:

1. Because the inclusion of a large fragment of testicle or liver or of an entire organ in a culture medium, or on agar, **always results, without exception**, in bacterial rot;

2. Because an identical result is provided by **Servel's** experiment (75), which consists of sacrificing a dog by bleeding, then placing a ligature on the vessels of the liver and kidney, then aseptically extracting these organs and immediately suspending them, by the thread of the ligature, in a 1% chromic acid solution. After 5 days, **Servel** observed that the peripheral part of the organs was hardened and normal, while their central part was in a state of manifest putrefaction, with a fetid odor, and showed a large number of bacteria, micrococci, and bacilli.

This experiment, which dates from 1874, does not involve any culture, any manipulation of the organs, is impeccable, is not subject to any contamination by heterogeneous bacterial germs, to any criticism, and succeeds 100 times out of 100.

Why did the Commission of the Biological Society left it in the shadows and ignored it instead of repeating it?

However, it should not have been unaware that the result of **Servel's** experiment is a fact well known to histologists who, to avoid it, fix tissues in small fragments or thin slices.

In summary, if, when immersed in a bactericidal liquid, chromic acid, a fragment of liver, kidney, or testicle putrefies in its central part, it suffers even more so when placed in a favorable culture medium;

3. I formally contest the accuracy and validity of the second and third conclusions due to the existence of the crucial colibacillary function and the constant normal presence, in the blood and in all parts of the body, of the bacterial elements of the organic colibacillus, in the state of micrococci, bacilli, and germinative masses which are the leukocytes.

This statement is made indisputable by microscopic examination of the white layer that covers the red blood cells after centrifugation of the blood. This white layer is made up exclusively of the various elements of the colibacillary function, that is, of the colibacillus, which are micrococci, bacilli, more or less long mycelial filaments, hematoblasts (platelets), small rounded germinative masses, various leukocytes, all of which are colibacillus elements.

The deposition of a sufficient quantity of these elements in a suitable culture medium is always followed by the development of a colibacillus culture, just as the culture of a fragment of milk clot or pulp tissue is followed by the proliferation of a bacterium of the colibacillus group.

The existence of the capital bacterial or colibacillary function of living beings is proof, beyond dispute, of the existence of microbes and, more precisely, of the elements of the colibacillus at all points of the testicle and any other region of the organism of a higher animal or of a bacterium of another species at all points of the organism of any lower animal or plant.

* * *

It is therefore established, by the preceding observations, that the conclusions of this control of **Portier's** experiments are inadmissible, this control having been carried out under unfavorable conditions, contrary to the most elementary rules of determinism and which, consequently, remove all value from it and render it completely illusory.

So illusory that these conclusions do not even dare to deny the normal existence of bacteria in the healthy organism and that they limit themselves to claiming "t*hat it is impossible to affirm their existence in the normal state*."

It is obvious that this conclusion has no other purpose than to cast doubt and suspicion on the results of **Portier's** experiments.

But another result, this one certain, has been reached; it results from the very terms of the conclusions of the report we have just studied: that is, two eminent bacteriologists, Professors **Martin** and **Marchoux**, did not succeed, in their experiments, in obtaining bacterial cultures by cultivating animal tissues stuffed with an enormous quantity of the bacterial elements of the colibacillus, whose presence is normal and constant, the latter being a constituent element of the animal organism and the active agent of the colibacillary function, which is as important as respiration and blood circulation.

This is where the defense of the false pasteurian dogma of the asepsis of living organisms has led Professors **Martin** and **Marchoux**.

Faced with such facts, one must ask how long we will continue to poison science with such procedures.

* * *

Regarding the results of **Portier's** experiments, I will conclude, following this critical examination:

1. That the facts observed and published by **Portier** are accurate;

2. That the microorganisms, cultures of which he obtained by seeding plots of living animal tissue, have no relationship with mitochondria, but only with the active elements of the bacterial function of living beings, the colibacillary function of higher animals;

3. That since mitochondria have no real existence and are only the scattered and shapeless debris of the cytoplasmic network and the dumbbell-building organelle, the latter, of a very particular form and already organized, is clearly distinguished from the bacteria cultivated by **Portier**;

4. That, consequently, the discussion which took place at the *Société de Biologie* on the subject of the assimilation of mitochondria to bacteria was inappropriate because it focused on two different subjects: on the dumbbell-building organelle on the one hand or the mitochondria which are its debris and, on the other hand, on the bacterial elements of the organic colibacillus to which correspond exclusively the microorganisms cultivated by **Portier**, and which he called "symbionts";

5. That these elements are not symbionts, but rather one of the two elementary organelles constituting the animal organism, that is, the one that is the active agent of the crucial bacterial function of living beings.

The second constructive organelle of living beings, whose existence escaped **Portier** and all cytologists, the dumbbell organelle, which forms the cell, is also of a bacterial nature. It multiplies like another bacterium, becoming Koch's bacillus, or cancer-causing bacillus, while retaining its dumbbell shape when its properties are altered.

* * *

Regarding the control of the results of **Portier's** experiments by the Commission of the Biological Society, I will conclude:

The control of **Portier's** experiments was carried out using unacceptable methods and conditions, and the conclusions of this control were written in such a vague manner that they only demonstrate a desire to exclude any precision and to cast suspicion on the facts controlled.

Such a procedure is an attempt to conceal from all and sundry the enormous and harmful error of the two pasteurian dogmas, an error, however, amply demonstrated by **Béchamp** for more than sixty years.

I once again draw the readers' attention to the extreme seriousness of such attempts to maintain in science such enormous errors, harmful to the general interest of all nations, harmful to the progress of science and to the fight against the most formidable diseases of man. Read in this book what concerns epidemiology, diphtheria, eruptive diseases, infantile poliomyelitis, the errors of bacteriology, and one will realize the heavy responsibility that falls on those who engage in such attempts against the public interest.

The demonstration of the bacterial nature of the animal organism, that of the existence of the capital colibacillary function or general bacterial function of living beings, animals and plants, achieve the definitive destruction of the pasteurian dogma of the asepsis of living beings, a dogma which is, unfortunately for the school that still seeks to save it, exactly the opposite of the truth, the very negation of the nature of living beings.

It was inevitable that a day would come when this capital error would be revealed and cause the collapse of harmful dogmas.

Whatever the defenders of these dogmas may do now, they will be powerless to reestablish them.

* * *

The facts, the truth, are now known, the light has been shed, and the right path is open; others will follow it.

I will conclude this history of the pasteurian school's systematic opposition to anything that could demonstrate the falsity of pasteurian dogmas by outlining the struggle that has been waged since 1926 against the results of this work and against their publication, a struggle that has taken on a new character: the suppression of freedom of opinion and publication.

V. THE STRANGLING OF FREEDOM OF OPINION, DISCUSSION, AND SCIENTIFIC PUBLICATION IN FRANCE

THE REAL MOTIVES THAT PROVOKE IT, ITS HARMFUL ACTION FOR THE GENERAL INTEREST, FOR THE PROGRESS OF SCIENCE, FOR THE FIGHT AGAINST DISEASES, AND FOR THE INFLUENCE OF SCIENCE AND FRENCH THOUGHT ABROAD

For almost twenty years, shameful and unacceptable acts have been committed, which have caused serious harm to the general interest of science and, in particular, to the fight against disease; they demonstrate such a degradation of the French spirit that it is necessary to make them known to the public and, in particular, to the medical world.

To this end, we will examine in turn:

1. The maneuvers against freedom of scientific opinion, discussion, and publication;

2. The influence of the suppression of this freedom on the general interest of science, and the effect of false dogmas on the influence of science and French thought in the world.

* * *

1. Freedom of scientific opinion, discussion, and publication

If someone tries to suppress freedom of opinion and publication in daily political newspapers, all journalists and the public will rise up as one to restore freedom. When it comes to science, it is quite different. When, in 1926, I wanted to publish in the *Academy of Sciences* and other learned societies the results of the research that were the subject of the first volume of the work: *Constitution of Animal and Plant Organisms; Causes of the Diseases That Affect Them*, this publication was refused.

In addition, an order was issued to the main medical journals to prohibit any publicity about my research. In a word, freedom of opinion, discussion, and publication was strangled, and, as a sign of the general blindness of minds, not a single scientist rose up,

neither at the Academy of Sciences, nor at the Academy of Medicine, nor at the Society of Biology, to protest against the strangulation of this freedom. A conspiracy of silence was established.

Who launched this shameful campaign against freedom of opinion and against the general interest of science?

It was Professor **Roux**, director of the Pasteur Institute, assisted in this task by a complacent and incompetent permanent secretary of the Academy of Sciences.

The reader will certainly wonder what was the motive that drove Professor **Roux,** director of the Pasteur Institute, to act in this way?

This motive is as follows:

It was the defense of pasteurian dogmas that are known to be false, even in this Institute, dogmas that they want to maintain against all odds, against the general interest of humanity, against the interest and progress of science, against truth and even against reason, in order to prevent or delay as long as possible the collapse of a shaky scaffolding, whose fundamental bases are false.

And it was necessary to try to defend these dogmas because the book I had just published destroyed them by establishing: that living beings, animals and plants, are of a bacterial nature; that the bacterial viruses of heterogeneous infectious diseases have their original source in the organisms of living beings and especially in plants which, for the most part, are human food; finally, that many bacterial diseases have an autogenous development.

These new notions had been established with a precise, progressive determinism, which made them indisputable. Therefore, no one even tried to discuss them. What had succeeded against others could not succeed this time, because the danger was much greater. So they rallied to the process of stifling them.

The progress of science, the general interest, the interest of the sick, all that is in play; only one thing matters, the material interest of an Institute, and to support this interest, it is necessary at all costs to maintain the false pasteurian dogmas, that is to say, error, and to combat the truth.

Above all, let no one believe that this is a question of a scientific order. It is exclusively a question of material interest

I tried in vain, through approaches and visits, notably to the director of the *Pasteur Institute*, to obtain an explanation, clarification, or even criticism. I found before me only people who avoided all discussion.

But I had better things to do than waste my time battling with such adversaries. I had before me a perspective full of promise and which, I could hope, would give me victory, even against the conspiracy of silence, and however powerful my adversaries might be.

In 1936, I published a second volume, in which I demonstrated part of **Villemin's** conclusions as false and in which was provided new peremptory proof of the falsity of the pasteurian dogma of the asepsis of living organisms by demonstrating the nature and autogenous origin of Koch's bacillus and the autogenous development of tuberculosis.

By a letter dated October 17, 1936, addressed to the Secretary General of the Academy of Medicine, Professor **Achard**, I asked him to make these results known to the Academy myself in a short memorandum, the text of which was attached to the letter.

Following this letter, on November 10, at my laboratory at the Museum, I presented the question with supporting projections to four members of the Academy who were part of the Tuberculosis Commission of this learned society, Professors **Bezançon**, **Roussy**, **Sergent**, and Doctor **Rist**. They did not raise a single objection to the facts presented to them.

I thus allowed them to observe, I can say, through projections of the photographs shown in the second volume of this work, the birth of Koch's bacillus and its formation by the embryonic cells of tuberculous tissue.

I was not aware that my four listeners had made any report to the Academy to inform it of the facts I had presented to them.

Under these conditions, I addressed the following letter to the Secretary General of the Academy:

Doctor J. Tissot, Professor of General Physiology
at the National Museum of Natural History,
to Professor Achard, member of the Institute,
Secretary General of the Academy of Medicine.

Paris, November 18, 1936.

Mr. Secretary General,

In my letter of October 17, 1936, and in the short communication attached to it, I presented the results of the long and patient research I have carried out on the nature and origin of tuberculosis and Koch's bacillus, and I proposed to make these results known to the Academy of Medicine.

On November 10, before a commission of the Academy composed of Mrs. Bezançon, Rist, Roussy, and Sergent, I presented the demonstrations and photographic proofs of

the facts listed in my communication; these are contained in a volume that I gave to each of them.

Unable to delay any longer making these results publicly known for the purpose of submitting them to the scrutiny of scientists, I have come to ask you to kindly inform me of the Academy's response to my proposal.

I wish to make it clear that the results I present are my sole responsibility, and that accepting their publication does not constitute any approval by the Academy and in no way prejudges their value. My proposal is solely intended to make known and submit to the scrutiny of scientists the results of a completely disinterested scientific work which, I provide proof, has achieved notable progress in the knowledge of the nature and causes of a social scourge: tuberculosis.

I felt it my duty, first of all, to present these results to the Academy of Medicine, whose essential goal is the dissemination and scrutiny of scientific work aimed at advancing the fight against disease.

Please accept, Mr. Secretary-General, the assurance of my highest consideration.

* * *

In response to this letter, I received the following:

ACADEMY OF MEDICINE 16,
rue Bonaparte (VI)

Paris, November 20, 1936.

Dear Sir and Honored Colleague,

The work you sent to the Academy was, in accordance with the regulations, submitted for examination by the Board of Directors.

Because of the importance of the discoveries you announce, the Board considered that it could not conclude on its publication without having been able to verify these facts.

However, it does not have the necessary research resources, so it could only give a negative conclusion to your request.

Please accept, Dear Sir and Honored Colleague, the expression of my highest consideration.

The Secretary General,
Ch. ACHARD.

Professor TISSOT,
Museum of Natural History,
7, rue Cuvier.

Thus, strictly according to the text of this letter, the Academy of Medicine **does not possess the research resources necessary to verify the facts submitted to it**.

I object to this assertion that it does have a series of commissions, all specialized, which it frequently entrusts with missions of this kind, and that it seems to me that if, following my request for communication to the Academy, four members of the Tuberculosis Commission came to hear in my laboratory the presentation of my research on Koch's bacillus, it was precisely with a view to such a verification, which they could, moreover, complete solely by microscopic examination of my preparations, an examination for which they were all specialized. The Academy was therefore truly, truly, put in a position to verify the announced discoveries, and this verification was, in fact, carried out by four members of its *Tuberculosis Commission*.

It was under such conditions that the Academy of Medicine made this astonishing decision:

"That it is precisely because of the importance of the discoveries I announced that it could only give a negative conclusion to my request for communication to the Academy."

Does this mean, taking this response literally, that the Academy of Medicine is obliged to refuse the communication of all important scientific discoveries because it is unable to verify them and that, consequently, it only authorizes unimportant communications? It is sufficient to invoke against the decision taken by the Academy the fact established in all learned societies that the authors of publications alone, and not these societies, are responsible for what they publish.

Moreover, the Academy of Medicine did indeed once publish, without checking them in the least, **Villemin's** false conclusions on the origin and nature of Koch's bacillus and tuberculosis. These conclusions had a harmful influence on subsequent research concerning this disease, particularly on the antibacterial therapy using chemicals that was used against it—creosote, which claimed so many victims, for example.

At that time, the Academy of Medicine honored itself by allowing complete freedom of publication to **Villemin's** opponents such as **Pidoux**, and later, to those of **Pasteur**. But since then, a new method has appeared, introduced into all learned societies by the pasteurian school and innovated by Professor **Roux**: the suppression of all new notions that undermine Pasteur's false dogmas.

The comedy that unfolded in 1936 at the Academy of Medicine is only a repetition, provoked by the pasteurian school, of the one that unfolded in 1926 at the *Academy of Sciences and the Biological Society*, on the occasion of the publication of the first volume of this work, a publication that renewed the destruction of pasteurian dogmas, first carried out by **Frémy** and **Béchamp** and a second time by **Portier**.

Let us add that not a single voice was raised at the Academy of Medicine to defend freedom of opinion and publication.

And it is our country, where freedom of scientific opinion is thus strangled, that is presented as the torch of freedom in general, of freedom of opinion in particular, and of scientific progress. In reality, the academies that hold this torch have transformed it into an extinguisher of scientific progress.

This book, which will penetrate all parts of the world, will enlighten world opinion on this subject.

Will not a single voice, not a single authority, in our country rise up to protest against such base, such condemnable procedures and to finally institute control over the unacceptable therapeutic procedures currently used?

* * *

In France, we do not forgive a scientist for having discovered what others have failed to see during 20 or 30 years of research, despite having had it before their eyes almost daily. In our country, such a discovery made by a Frenchman unleashes hatred, while we swoon with admiration over the work of foreigners who achieve only minimal scientific progress, often none at all, and sometimes even introduce empirical and disastrous therapeutic methods.

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Here is proof that I am not exaggerating: a young biologist, entrusted by his teacher with the mission of demonstrating the absurdity of my conclusions on the nature and origin of Koch's bacillus, was naive enough to write, in his attack (Rev. gén. des Sciences, March 3, 1937):

It is difficult to admit that the same technique, applied to the same material, would have shown Mr. Tissot alone new facts that a singular, very disturbing and truly inexplicable coincidence would have allowed all of his predecessors to miss.

This cry of protest against the one who discovered what others did not see, clearly shows that this is something that is not forgiven in France. Is it then this time indignation that this present volume will arouse because I have seen what thousands of doctors from all countries, notably from Pasteur institutes, have not seen and have even denied

the constitution of the virus and the false diphtheria membrane by *Cladosporium herbarum* and other hyphomycetes of cereals, the multiplicity of diphtheria viruses, the real hyphomycete virus of syphilis that I isolated, the exact nature of the fibrinferment which is the micrococcal form of the organic colibacillus, the capital colibacillary function, general bacterial function of all living beings of the two kingdoms, of which thousands of doctors have had the elements in hand, before their eyes, and which they have not seen, however, because their minds were sterilized by the false pasteurian dogmas, the knowledge of the two elementary organelles constituting living organisms, the knowledge of the halter organelle, organelle constructor of the cell and tissues, the error of mitochondria, etc.

* * *

At that time, only two men, two of my colleagues from the Museum, spoke out against the attitude of learned societies, not to support my cause, but only to protest against the strangulation of freedom of scientific opinion and publication. Their gesture honors them all the more since they were the only ones to have the courage.

On the one hand, **P. Rivet**, Professor of Anthropology at the Museum, requested the organization of a conference in the Museum's main amphitheater in which I would explain the autogenous origin of Koch's bacillus and the autogenous development of tuberculosis, and to which the entire scientific elite of Paris would be invited.

On the other hand, **R. Anthony**, Professor of *Comparative Anatomy at the Museum*, opened the columns of the *Revue Générale des Sciences* to me, and gave a lecture at the Sorbonne to fight against scientific dogmas in general and against the strangulation of freedom of opinion.

At the end of my lecture at the Museum on February 25, 1937, a listener came down to the bottom of the amphitheater and asked to speak to prove in five minutes, he said, that the research I had just presented was nothing but rubbish (sic), which provoked protests from the audience. Very calm, I limited myself to first asking this listener if he had read my book, a question to which, very embarrassed, he naively replied: "I haven't read it, I've only skimmed through it," a response which provoked a new protest from the audience, and which was enough to judge the nature of the mission with which he was charged.

I add that this auditor, as well as another who accompanied him, were part of the staff of the Institute mentioned above, and that a large group of students from this same Institute accompanied him; according to letters that auditors wrote to me the very next day, these students, grouped in such a way that the preparation of a demonstration of hostility was obvious, made themselves noticed by shouting with their fists raised.

One auditor, Doctor **Fougerat**, wrote to the director of one of the largest medical journals:

... This explosion of sectarianism suffocated me; it was the hatred that contorted faces against this great modest man, ***Tissot****, a real band of unchained savages, right up to the peaceful Y... who stood out for his violence, brandishing his umbrella like a tomahawk.*

And this explosion of hatred immediately convinced me: ***Tissot is touching on something other than a scientific question; on considerable material interests that would be ruined if his concepts*** *turned out to be real.*

You can be sure that the conspiracy of silence will redouble and that everything will be done to break the man. In matters where only study, courteous and exhaustive discussion, in the love of truth, should, yes, alone, have the right to be heard and to reach a conclusion, this is proper.

The fears of the author of this letter were exaggerated. It is in no one's power to break me, for no other motive guides me than the progress of science and the interests of humanity, and, moreover, I am not one of those who weaken and become discouraged.

The contents of this book are the best proof I can provide.

* * *

My colleague **Anthony** having opened the columns of the *Revue Générale des Sciences* to me, I exposed the results of my research on the origin and nature of Koch's bacillus and tuberculosis (second volume).

The campaign of cover-up immediately followed against **Anthony** and against me. A biologist, a professor at the Sorbonne, was charged with carrying out this cover-up. He, in turn, charged a head of his laboratory, the young biologist mentioned above, still not very expert in the art of criticism, who tried to demonstrate the absurdity of my conclusions on tuberculosis.

He wrote a long article in the same journal (83) in which he demonstrated only two things: his incompetence in the matter and his lack of critical thinking. The criticisms he formulated do not deserve to be included here. I refuted them properly in the *Revue Générale des Sciences* (78).

In this refutation, I had to, without naming him, implicate this young biologist's teacher in order to teach both of them what scientific error consists of, by citing to them, as an example, one of the most egregious ones committed during this century. This prompted him, this time, to launch the attack himself, which he did in a violent article in the *Revue Générale des Sciences*, where he wrote, in particular (7):

When one claims to observe mitochondria using the techniques employed by Mr. Tissot, one is making the equivalent of a gross miscalculation, and everything one deduces from it is strictly worthless and deserves no discussion, and especially no publicity.

"*Deserves no discussion, and especially no publicity*," this, naively admitted, is the sole goal of this campaign with which the defenders of the false pasteurian dogmas had charged this professor: stifling. He added, moreover, that my conclusions are a challenge to contemporary biology.

This present third volume will teach this biologist that mitochondria, with the shapes and properties attributed to them, do not exist and correspond only to gross errors of observation; that consequently, he was unaware of what he was discussing: that this error will remain one of the grossest of the century and that it is indeed this that is a challenge to contemporary biology.

I wrote a very precise response to this biologist's attack; it was composed and I corrected the proofs; but it never saw the light of day; I indicated why, on page 146 of this volume, where this whole discussion is set out in detail; I will not return to it here, the aim of this chapter being exclusively to show in what shameful, scandalous way freedom of opinion, of scientific discussion and publication is being strangled in France, which is claimed to be the country of liberty; to show, moreover, the immoral action on the one hand, and the anti-French action on the other, of those who do not fear to engage in such maneuvers exclusively to perpetuate harmful dogmas which they know to be false and this for the sole purpose of protecting material interests.

I say anti-French action because, by seeking to perpetuate these false dogmas, definitively destroyed in this book and which the whole world must soon know are outdated, they cause considerable harm to the influence of French science and thought abroad.

It is French science that introduced and propagated throughout the world the enormous and harmful errors of pasteurian dogmas: it is it itself that must reestablish the truth.

This relentless struggle against my efforts to make this truth known has continued every where, continuously, to this day and even in the Museum, through smear campaigns undertaken with the aim of isolating me, of discouraging any worker who would have wished to study in my laboratory, in a word, with the aim of condemning me to sterility by depriving me of all assistance.

The Commissions of the Scientific Research Fund, particularly those of Biology and Experi mental Medicine, were prejudiced against me, as was the director of the Research Fund; every request I made was rejected; I was refused technical assistance, but several were granted to people who only used them to establish a deceptive facade, or to foreigners who didn't even use them!

The Department of Biology and Experimental Medicine was prejudiced against me, as was the director of the Research Fund; every request I made was rejected; I was refused technical assistance, but several were granted to people who only used it to establish a deceptive facade, or to foreigners who didn't even use it!

The Research Fund, created to promote scientific research! What a mockery! It has been above all an instrument of influence in certain hands, an instrument used primarily to serve, not science, but particular interests; it is one of the causes that have caused the disappearance of the freedom of thought of those who engage in scientific research.

Despite this fierce struggle against me, they have not succeeded in reaching me. They have certainly succeeded in preventing me from developing what this book contains with all the scope that was appropriate, particularly for the research and control of the original source of infectious disease viruses.

But my research could not be stopped, because it is not large sums of money, multiple very expensive devices, or very large laboratories that are necessary to make important discoveries. What is necessary is selflessness, common sense, critical thinking, hard work, a passion for scientific research, and, above all, freedom of mind and the courage to assert one's opinion.

With these means, a man is unassailable. I was studying everything this book contains, with the certainty of annihilating the false dogmas that halt the evolution of science and of arriving at the great principle that will lead to the elimination of infectious diseases and epidemics, diseases that are man's greatest enemies: typhus, plague, diphtheria, eruptive fevers, typhoid fever, poliomyelitis, etc.

This goal has been achieved. That is why I was and am unassailable.

The struggle to try to stifle the notions contained in this book, to try to perpetuate false dogmas, will certainly not disarm, but it will be powerless against the truth now known. Despite this unspeakable struggle, and whatever the defenders of false dogmas may do, in the not-too-distant future, most infectious diseases and epidemics will have disappeared, without the use of vaccines or serums and through simple food hygiene regulations. I say that this goal will be achieved in the not-too-distant future because I am

certain that, among all nations, there will be at least one where the harmful influence of the Institute I incriminate here will not be exerted and which will apply, for the general good, the principles developed in this book.

The impact of suppressing freedom of opinion and publication on scientific progress.

The effect of false dogmas on the influence of French science and thought abroad.

It is obvious that the fierce struggle pursued against the results of my scientific research since 1926 does not target me personally and is motivated solely by the fear that the falsity of pasteur's dogmas, whose defenders themselves certainly no longer doubt, will spread throughout the world and cause the collapse of a shaky edifice, particularly of a dogmatic bacteriology that has remained in its infancy and in error.

There will be no shortage, in a certain scientific circle, of attempts to influence public opinion by crying scandal because I attack the dogmas and principles established by **Pasteur**; there will be no shortage of people opposing his worldwide fame, the greatness of his work, and perhaps even of accusing me of anti-French action.

We can foresee anything in our country where, for three-quarters of a century, we have succeeded in making the medical world and the masses believe, according to this colorful expression, "*that bladders are lanterns*."

To those who would use such arguments, I will respond as I will explain: In science, neither the personality nor the reputation of men have the slightest weight.

They are all subject to error, and every scientific error must be destroyed, wherever it comes from, and replaced by the truth. Will it be claimed, because it is **Pasteur**, that the false and harmful dogmas he established must be respected?

On the other hand, am I the first to combat these dogmas?
Haven't **Frémy**, **Claude Bernard**, **Béchamp** already peremptorily demonstrated their falsity? What is surprising is that, to this day, the peremptory demonstrations of these scholars have been extinguished and that, throughout the world, people have been able to give credence to these false dogmas.

The result of these dogmas has been the total halt of progress in the fight against autoge nous and heterogeneous diseases, of progress in bacteriology and other branches of biology. It was therefore necessary to destroy them by new demonstrations, of such precision that their defense would become impossible.

* * *

It is now proven that those who have defended and perpetuated the dogmas of atmospheric panspermia, of the asepsis of living beings, of monomorphism and of contagion have created, for those who believe in them, and first of all for themselves, the impossibility of achieving knowledge:

1. Of the bacterial function of living beings, a function which, for higher animals, is the colibacillary function;

2. Of the real and original form of viruses;

3. Of the nature and original source of heterogeneous viruses;

4. Autogenic diseases, due to a deviation of the coli-bacillary bacterial function (diseases caused by colibacillus, streptococcus, staphylococcus, etc.);

5. The function of the universal constructor organelle, the deviation of which constitutes tuberculosis and cancer;

6. The nature of the ferments of animal and plant organisms;

7. The mode of development of heterogeneous diseases and epidemics, their causes, and the means of avoiding them.

The sole dogma of asepsis of living beings made it impossible to understand any of these seven notions of such capital importance, on which the entire future of the fight against human diseases depends.

How could those who defend pasteurian dogmas have achieved knowledge of the colibacillary function of animals, since the dogma of the asepsis of animal organisms forbids them from admitting that a normal and constituent element of the organism could be of a bacterial nature?

How could they have discovered that human plant foods are the principal original source of heterogeneous viruses, since the same dogma forbids them from admitting the normal existence of bacteria in plants and their quality as constituent elements?

It cannot therefore be disputed that these dogmas have had harmful consequences for the biological and medical sciences.

Under these conditions, have those who have perpetuated them until now served the interests of French influence in the world? These gross errors that I have pointed out could not fail to become known one day or another, and the obvious result of the cover-up campaigns undertaken by the Institute concerned would have been, inevitably, that it was from abroad that the demonstration of the falsity of pasteurian dogmas would have come.

The fact that this demonstration and the reestablishment of the exact principles that must replace the errors are the work of French science, is it not more likely to develop French influence abroad than the perpetuation of gross, enormous errors of harmful influence, for which a French school has assumed full and exclusive responsibility?

Finally, what will be the effect on this influence abroad of the suppression of freedom of opinion and scientific discussion in France, of attempts to stifle new notions in order to maintain false dogmas, inadmissible acts among a people who pride themselves on setting an example of freedom of mind, of freedom in general, and of the search for scien tific progress?

* * *

CHAPTER VIII

BACTERIAL FUNCTION OF LIVING BEINGS: COLIBACILLARY FUNCTION IN VERTEBRATES.

COLIBACILLUS IS A NORMAL AND CONSTANT CONSTITUENT OF THE ANIMAL ORGANISM; IT IS WHAT FULFILLS THE CHEMICAL FUNCTIONS OF A FERMENTATIVE NATURE.

In the first volume of this work, I have already presented the results of a study of colibacillus (page 478) carried out by experimenting exclusively on pure cultures of human origin, which I transformed into *Citromyces* and *Penicilium*.

From the identity of these two conidial forms with those obtained by culturing the living matter of the human organism, I concluded that the colibacillus of man is an abnormal and bacterial form of his organic mold and that, consequently, colibacillus infections are autogenous diseases.

This research having been published in 1926 (first volume), I resumed and completed it later. This new study confirmed that colibacillus is one of the forms of living matter in the animal organism and established that it appears there, not in an abnormal form of transformation, but in its normal bacterial form, that it is a constant constituent element and that it exercises a physiological role of the highest order, indispensable to the manifestation and preservation of life.

Here are the facts that led me to the knowledge of the nature of colibacillus and its functions in the animal organism:

1. Colibacillus always exists in the intestine of the newborn immediately after birth and before any ingestion into the digestive tract;

2. Colibacillus exists normally and constantly in the blood of animals in both bacillary and granular forms.

3. Colibacillus normally exists in connective tissue and in all animal organs; it is to its granular elements that tissue juices owe the property of accelerating blood clotting.

4. The various elements of colibacillus multiply in blood, plasma, or purified fibrinogen solutions; it is this multiplication that causes the spontaneous coagulation of the latter.

5. One of the functions of organic colibacillus is the formation of certain connective tissue elements.

6. The granulations, platelets, and bacilli, which constitute the white layer that covers the blood cells after centrifugation of oxalated blood, give rise to a culture of colibacillus if they are inoculated in broth.

7. The granular elements of colibacillus are the fibrinferment or serozyme. These elements also have proteolytic, glycolytic, lipolytic, and other properties.

Here now is the demonstration of these facts:

* * *

1. Colibacillus always exists in the newborn's intestine immediately at birth and before any ingestion into the digestive tract.

This fact, well known and uncontested, constitutes one of the peremptory proofs of the falsity of the dogmas of atmospheric panspermia and the asepsis of living organisms. Those who wish to defend and perpetuate these dogmas indefinitely will not fail to

claim that the colibacillus of the newborn's intestine comes from the mother's intestine, from where it entered the blood, then from the mother's blood into the newborn's blood, and from the latter into the intestine; but this explanation is untenable, because:

1. The defenders of the dogmas can only consider the colibacillus of the mother's intestine as being of heterogeneous origin.
2. They cannot deny that the presence of colibacillus in the mother's intestine is constant;

3. They cannot consider the passage of this colibacillus from the intestine into the blood and vice versa as anything other than a pathological phenomenon, a blood infection;

4. They cannot dispute that the presence of colibacillus in the intestine of the newborn, from the first hour, and before any absorption of liquids and food, is constant and normal;

5. They must therefore necessarily conclude from their reasoning that there is always, in the pregnant animal, colibacillus infection of the blood, and that the same colibacillus infection is always transmitted to the blood of the newborn, from where the colibacillus passes into the intestine.

Thus, by invoking the two false dogmas mentioned above, we arrive at the absurd conclusion that a constant phenomenon, which presents no exceptions, which is universal, and which can therefore only be considered a normal phenomenon, is nevertheless an abnormal and pathological phenomenon, even though the individuals who present it,

the mother and the newborn, are perfectly healthy, do not suffer from it at all, and retain this supposed intestinal and blood infection by colibacillus throughout their lives.

Thus, this absurd reasoning, which would not have been missed if I had not accumulated the peremptory proofs set out later, succeeds, contrary to its purpose, in demonstrating the accuracy of this new notion of capital importance: the normal existence, in the animal organism, of a bacterial constituent element fulfilling physiological functions indispensable to life.

This new notion constitutes the destruction, the definitive annihilation of the pasteurian dogmas of atmospheric panspermia and the asepsis of living organisms. It seems obvious that the defenders of these dogmas will attempt to annihilate this new knowledge or at least to stifle it for a long time, but this is now impossible because the demonstrations made are peremptory, and they will inevitably be known; the dogmas are dead and they will not be resurrected.

2. Colibacillus exists normally and constantly in the blood of animals in both bacillary and granular forms.

To verify this, we centrifuge dog blood, for example, oxalated at 2 per 1,000 immediately after leaving the vessel. With a pipette, we carefully aspirate the thin white layer above the red blood cells, mix it with salt water at 6 or 7 per 1,000, then centrifuge again in a tube much smaller than the first, so that the white layer is quite thick, above the red blood cells carried along with it; we take a small quantity, spread it on a slide, and examine it under a coverslip or directly, after evaporation, fixation by heat and staining with dahlia. In such a preparation, we observe:

1. Bacilli that have exactly the appearance of the colibacillus (figs. 6 and 8, pl. 74). They are of variable length and few in number; they are 0.5 to 0.7 microns wide and 1 to 5 microns long. Some are arched, others shuttle-shaped (fig. 11, pl. 74); finally, we observe notably thinner bacilli that are only about 0.3 microns wide, and often have 1 polar globule at each end (figs. 3 and 4, pl. 74);

2. Basophilic mycelial filaments not yet segmented into bacilli (figs. 9, pl. 74);

3. Granulations of 0.2 to 0.5 or 0.6 microns in diameter, some basophilic, others achromatic or barely colored; they constitute the serozyme (fig. 6, pl. 74);

4. Rounded, irregular masses, entirely basophilic, from 2 to 20 or 30 microns or even more in diameter (fig. 10 to 13, pl. 74), the smallest appearing clearly formed by large basophilic granules; these are all germinative masses, the large masses giving rise to

mycelial filaments which segment into bacilli; the smallest masses form filaments which segment into bacilli thinner than those of the large masses;

5. Rounded elements 2 to 5 microns in diameter, irregular in shape, consisting of groups of 6 to 12 granules, each capable of germinating, each giving rise to a thin filament.

These elements are hematoblasts or platelets. They are formed by granules identical to those mentioned above (see 3rd). Indeed, we will see later, in the study of the mechanism of blood coagulation, that platelets have an action identical to that of isolated granules, which, with them, constitute serozyme. Tissue juice also contains a large quantity of serozyme granules, to which it owes its coagulating properties. No more than platelets do they constitute cytozyme, as **Bordet** stated; this is constituted by blood lecithin.

The following experiment allows us to confirm the presence of a considerable number of bacilli in the blood: it consists of sensitizing a dog by one or more intravenous injections of horse serum. After 12 to 15 days, when the dog serum has acquired the precipitating and agglutinating power, the addition of a few drops to fresh normal horse serum causes flocculation. The flakes that appear there, examined under high magnification, appear to be made up either of large masses of tangled filaments, in the process of segmenting into bacterial elements, or of large masses of granules emitting a fine divaricate filament on which other fine, short-stalked granules (zymospores) are born; these latter elements will be studied later. These are the normal colibacillary elements of horse blood which, when injected into the dog's blood, have developed a culture and a special fibrin which, when transferred to the normal horse serum, agglutinates the colibacillary elements (figs. 5 and 6, pl. 122, 1st vol.).
In summary, bacterial elements are found in the normal blood of all animals in the bacillus and micrococcus forms; it is above all by examining the clot (figs. 1 to 4, pl. 74) that we become aware of the enormous number of these elements contained in the blood.

This is therefore a direct demonstration of the error of the dogma of the asepsis of the animal organism. The constant presence of these elements in the blood of animals, their enormous number, the functions of capital importance that they fulfill there show that they are a normal constituent element of the organism and that it is not possible to consider them neither as accidental, nor as saprophytes or symbionts.

3. Colibacillus normally exists in connective tissue and in all animal organs; it is to its granular elements that tissue juices owe the property of accelerating blood coagulation.

Let us examine a preparation of connective tissue, such as that in Figure 3 of Plate 37, first volume, which shows the intermuscular lamellar tissue of the ox colored by dahlia.

We see there a large number of scattered granulations which belong to the colibacillus and which are figurative elements identical to those of the serozyme or fibrinferment; it is these which give to the lymph which flows from the walls of wounds the power to accelerate coagulation; this liquid, which constitutes the tissue juice which has been called cytozyme and which, by mistake, has been believed to be the second constituent of thrombin, is in reality identical, in constitution and in action, to the serozyme. We see, on the other hand, rounded masses of various sizes, sometimes of irregular shape; the four or five largest masses are formed:

1. In their peripheral region by highly refractile hyaline granules of various sizes, stainable only with acid colors, such as eosin;

2. In the center of these masses, we see other granules, strongly stained by dahlia, therefore basophilic, which constitute the nucleus.

Most of the other rounded masses in the figure are entirely basophilic; the granules that constitute all these masses, large and small, emit filaments that are connective tissue fibers.

What is the nature of these masses of granulations? In the first volume of this work, I thought that some of them, entirely acidophilic, were mastzellen or nutritive cells (fig. 3, pl. 27, 1st vol.); that others, entirely basophilic, are the origins of connective fibers, and finally that the largest, entirely similar to blood leukocytes except for their size, also have the same purpose.

Knowledge of the existence and functions of the colibacillus in animal organisms also provides us with knowledge of the nature and origin of these masses and of leukocytes in general.

The granular masses described above are indeed leukocytes, and leukocytes are agglo-me rated masses of the granulations or spores of the organic colibacillus. The masses of granulations called hematoblasts or platelets are also leukocytes in formation, which will probably grow by the addition of other elements. This explains why platelets have been found to have a chemical constitution similar to that of leukocytes and, for the latter, an action similar to that of platelets on blood clotting.

On the other hand, knowledge of the organic colibacillus leads us to consider the various species of leukocytes as non-existent, and this for the following reason: a coli bacillus, totally basophilic at the beginning, progressively loses its basophilic matter; the spores it contains also lose it, and only a hyaline matter remains, which is colored only by acid colors, while the chromatin they previously contained was colored by basic

colors. It is the same for isolated spores which, always basophilic at the beginning, become neutrophils when they have lost the greater part of their chromatin, and acidophiles when they have lost it completely.

Every living figurative element is made up of a supporting carcass or skeleton, made of a hyaline, transparent, achromatic, and acidophilic material, and a basophilic substance, chromatin, which is the active substance of the element. This explains why leukocytes have acidophilic, neutrophilic, or basophilic granules. Any element rich in chromatin is basophilic at birth, then neutrophilic when it loses a large part of its chromatin, and finally acidophilic when it has completely abandoned the hyaline carcass.

The agglomeration of elements into a mass is the first stage in the development of a new generation. The agglomerated mass comprises achromatic elements, to which are joined elements loaded with chromatin and which, through multiple copulation, will form a germinative mass that will give rise to new mycelial filaments (see vol. 1, p. 34 and following).

Studying these phenomena, I had succeeded, before 1926, in definitively demonstrating the formation of true leukocytes in in vitro bacterial cultures and I had clearly understood and concluded, from that moment on, that the mode of formation of leukocytes in the animal organism takes place in an identical way, that is, by agglomeration of bacterial elements.

Indeed, I had concluded (p. 36 and pl. 1 and 6, 1st vol.) that:

The germinal masses of bacterial cultures do not differ from leukocytes. The normal leukocytes of the animal organism are born by the same process of formation as those of "in vitro" cultures and, like them, come from the segmental and spore-forming elements of a mold, the specific mold constituting the organism.

And, in support of my conclusion, I produced the photographs of plates 1 and 6 of the first volume, which demonstrate both the identity of the germinative masses of bacterial cultures with leukocytes and their mode of formation.

The new knowledge of the nature and origin of the bacterial elements (granulations and bacilli) of the animal organism and of the existence of the organic colibacillus provides this conclusion with a striking and peremptory confirmation; it establishes that leukocytes are indeed agglomeration or germinative masses of a bacterial culture whose elements all exist in the blood and which evolves according to the four phases determined in the first volume (p. 34): agglomeration, fusion of the agglomerated elements, germination, segmentation of the developed mycelial filaments.

It will be shown later, by another method, that the granular elements of the organic colibacillus are indeed the origin of the connective fibers.

4. The various elements of the organic colibacillus multiply in blood, plasma, or purified fibrinogen solutions; it is this multiplication that causes the coagulation of the latter.

Oxalated plasma and fibrinogen solutions in uncalcified salt water coagulate spontaneously. **Bordet,** who sought to elucidate the mechanism of this coagulation, demonstrated that the addition of gelatinous tricalcium phosphate to the fibrinogen solution followed by centrifugation greatly delays the moment of coagulation. Nevertheless, it still occurs; it begins with the appearance of small whitish flakes that float in the liquid and slowly grow.

I studied the mechanism of the formation of these flakes and, to do this, I first sought to determine the cause of the delay in coagulation caused by the addition of gelatinous tricalcium phosphate.

Examining a solution of fibrinogen under a microscope, I noticed that this substance carries with it in its precipitation a large number of granules of 0.2 to 0.6 microns and even larger, and that their quantity decreases after each addition of tricalcium phosphate followed by centrifugation. This fact therefore proved, firstly, that it is this rarefaction of the granules that causes the delay in coagulation and, secondly, that it is these granules that determine it. It is therefore they that constitute the serozyme, a ferment which, according to **Morawitz, Fuld, Spiro,** and **Bordet,** constitutes thrombin by its union with a second substance, the cytozyme.

I then studied the microscopic constitution of the white flakes that appear in unrecalcified oxalated plasma that has not been purified by the addition of tricalcium phosphate.

A flake, which has appeared for two days in oxalated plasma, is then placed on a glass slide, drained, and then a drop of water is added. It is drained again, carefully fixed by heat, and finally colored with dahlia.

Microscopic examination of such flakes shows that they are made up of a mass of tangled mycelial filaments that come in two forms:

1. In the largest and most opaque flakes, by long, partitioned, very basophilic mycelial filaments, on which bacilli 3 to 5 or 6 microns long are born (fig. 5, pl. 74); in the tangle of filaments, spherical fibrinous masses are noted (fig. 3, pl. 11, 1st vol.); finally, in the

meshes formed by the filaments, a large quantity of granulations and free bacillary elements is noted (see under the microscope pl. 117, 1st vol.);

2. Another region, more transparent and gelatinous in appearance, is made up of very long, tangled elements, which are chains of bacilli; all contain spores of various sizes, some very large, achromatic and very refractile, others more or less basophilic, which shows that originally all were basophilic, but that the chromatic matter gradually leaves them.

Some filaments are cylindrical and the segmentation into bacilli is not yet visible (Fig. 3, Pl. 74); in others, it is clearly marked and in a certain number, the bacilli are already in the process of separating and have their definitive form (Fig. 3 and 4, Pl. 74) which is cylindrical with both ends rounded, or with one or both ends tapered, or shuttle-shaped; they have a diameter varying from 0.3 to 1.2 microns and a length of 1 to 6 microns. They contain three or four refractile or more or less basophilic spores, two of which are located at the ends and are polar globules (Fig. 3 and 4, Pl. 74). The basophilic material that remains lodged between the spores gives the colibacillus its familiar appearance, especially when it is stained sparingly. The isolated filaments and bacilli are in fact the organic colibacillus of the horse.

Some bacillary elements are much larger than in broth cultures because here they have grown in their natural environment, plasma.

When the serozyme (white layer of centrifuged oxalated blood, platelets) is transferred to broth, it reproduces a culture of colibacillus; this point will be examined later.

When a certain number of flakes are thus formed in oxalated plasma or in an unrecalcified fibrinogen solution, the granular elements they contain (spores) spread into the liquid, multiply in turn, and produce mass coagulation. The greater part of the clot then has a constitution identical to that of the whole blood clot. Like the latter, it is made up of granulations from which emanates a fine, short, divaricate filament that carries fine, short-pedunculated granules (zymospores), which are new elements of the serozyme in the process of development. These granulations and divaricate filaments, entangled and bound together by the fibrin deposited on them, constitute the framework of the clot, the meshes of which are occupied by the serum or liquid deprived of fibrinogen.

It was demonstrated in the first volume (p. 185), for barley diastase and pork pepsin, that this germination of granulations is the process of multiplication of living figured ferments.

The demonstration of the existence of these granulations in the blood of animals, their nature (spores of the colibacillus), and their mode of germination, is of capital importance for understanding the cause of serum sickness and for understanding the mechanism of anaphylactic shock. These facts bring us the new notion that the injection into man or an animal of serum from another species constitutes an infection by the organic colibacillus of that species; they inform us about the immediate cause of the urticaria that appears as a symptom of this infection and which is the germination and arrest in the capillaries of the colibacillus elements of the antigen animal. This question will be studied later, with regard to anaphylaxis.

5. One of the functions of the organic coliform bacteria is the formation of certain elements of connective tissue.

The microscopic examination of the flakes which appear in the oxalated plasma provides still further indications of capital importance to which I have already drawn attention in the first volume.

Within the total plasma clot and within the primitive flakes, there are large basophilic masses and large clusters of achromatic granulations, each of which emits a long, almost achromatic hyaline filament. The collection of these more or less parallel filaments immediately appears identical to a bundle of elastic fibers. Elsewhere, we see a tangle of large branches, ramified bundles, all immersed in a mass of fibrin (pl. 119, 1st vol., and fig. 4, pl. 37 *ter* of this 3rd vol.). There is no doubt that this is real connective tissue, developed in a solution of fibrinogen by the evolution of the spores of the organic colibacillus.

The picture presented in the figures on plates 119 of the first volume and 3, plate 37 *ter* of the third volume, is exactly that of the organization of the clot in a wound. The repair of this wound is therefore carried out by the bacterial elements of the organic coli bacillus of the blood clot, leukocytes being the main ones among these bacterial elements.

Nageotte, who studied the organization of the clot and the repair of wounds, published in his book *The Organization of Matter* (52) two very interesting figures (figs. 5 and 6, pl. 2) relating to the following experiment: he makes, aseptically according to him, a crude clot of dog blood in a small test tube of collodion which he introduces into the peritoneum of another dog where it remains for 8 days.

In Figure 5 of Plate 2 of his book, he shows the constitution of the clot at its formation; we notice, in the upper part, a germinative mass colored dark blue, giving rise to a dozen mycelial filaments; he calls this formation a fibrin star and therefore considers these

filaments to be made of fibrin; they are connective fibers emanating from the granulations of a mass germinative and they will remain perfectly distinct from the fibrin;

the latter does not and cannot form formed elements; only the formed elements of the organic colibacillus have this power.

In Figure 6, Plate 2, **Nageotte** shows the state of the clot after a stay of 8 days in the dog's peritoneum. We notice the large mycelial filaments mentioned above and which are of a much larger size; they are simple or compound mycelial filaments equivalent to certain branches of Figures 1 and 2 of Plate 119 of the first volume of this work; they are not direct formations of fibrin as **Nageotte** thinks; they are living mycelial elements, formed by the large germinative masses.

There is also the formation in the clot of filaments segmented into bacilli, as evidenced by certain colibacillary elements present in the fibrin: for example, a shuttle bacillus still equipped with a polar globule, located in the lower left corner of the figure, 11mm above the lower edge, and 8 mm to the right of the left edge. It is emptied of its basophilic substance. Another, in the form of a coccobacillus, still quite clearly equipped with its two polar globules, is located halfway up the figure and 3.5 mm from its left edge; the figure contains still others.

Nageotte, a convinced supporter of the accuracy of pasteur's false dogmas of the asepsis of living organisms and atmospheric panspermia (see pp. 110, 111 of his book), could not see these bacterial elements nor, if he had seen them, could he discern their significance: the organization of the clot, the repair of wounds, the partial formation of connective tissue being the work of the bacterial elements of the organic colibacillus, his belief in false dogmas prevented him from understanding them.

* * *

In summary, here are established new notions of considerable biological importance:

1. The existence of the colibacillus as a constituent element of the animal organism;

2. The fermentative properties of its granular elements;

3. The mechanism of the formation of fibrin, a tissue-repairing substance, by hydrolysis of lecithins carried out by the granular elements of the organic colibacillus;

4. The formation of certain elements of connective tissue by its granulations.

6. The granulations, platelets and bacilli, which constitute the white layer that covers the blood cells after centrifugation of the oxalated blood, give rise to a bacterial culture if they are inoculated in broth.

Let us repeat exactly the same operation as before to carefully collect the white supra-globular layer with a sterile pipette, but this time with horse blood collected at the slaughterhouse in a sterilized vessel containing the necessary quantity of sterile sodium oxalate solution; the blood is collected immediately as it leaves the animal's vessels. All aseptic precautions are also taken for the centrifugation of the blood and then for that of the white layer transferred to salt water.

A small quantity of this white layer, inoculated in glycosylated peptone broth, gave the culture whose elements are represented in figures 5 and 6, plate 121, 1st vol. We note:

1. Bacilli about 0.5 microns wide and 1 1/2 to 4 long, very basophilic;

2. A type of thin bacillus, 0.2 to 0.3 microns wide, generally containing one or two polar globules;

3. Bacilli of sizes intermediate between the two previous ones;

4. Some slightly shuttle-like bacillary forms;

5. Large granules, the largest highly chromatic, 0.5 to 1 micron in diameter and resulting from the segmentation of long bacilli;

6. Finer granules, about 0.2 to 0.5 microns, some very basophilic, others less so or achromatic.

These various elements are exactly the same as those in colibacillus cultures. To see many long bacilli, one must examine the culture or its first subculture from the beginning of its development. This is indeed the colibacillus; in fact:

1. The culture ferments glucose and lactose;

2. It quickly turns litmus agar red;

3. When inoculated in a 2% peptone solution, it forms indole;

4. When inoculated in milk, it coagulates it.

7. The granular elements of the colibacillus are the fibrinferment or serozyme; they also possess glycolytic, lipolytic, proteolytic, and other properties.

The nature of the fibrinferment was fully demonstrated in the first volume of this work, published in 1926. By then, I had established that the fibrinferment is a single body and not formed of two components, the serozyme and the cytozyme. The fibrinferment is

exclusively the serozyme, and the two words refer to a single object. The cytozyme has been inaccurately called by this word; two different things have been called cytozyme:

1. A body contained in the blood and tissue juice and which has been considered a lecithin;

2. The tissue juice, in block, or the blood platelets or an extract of these platelets (**Bordet**).

This is a fundamental error. Tissue juice does indeed contain lecithin, but it mainly contains the micrococcal elements of the colibacillus, that is, the active elements identical to those that constitute the serozyme; as for the platelets, they are the serozyme itself in the form of agglomerated elements, and not the cytozyme. This is an error of **Bordet's** which was for him the cause of results that he judged inexplicable, but whose cause I indicated in the chapter on Blood Coagulation.

In short, tissue juice contains, like blood, the two elements necessary for the reaction that causes fibrin to coagulate: colibacillary cocci and lecithin, and by this constitution, its action is rigorously identified with that of serozyme; this is why it reinforces the latter's action. In summary, cytozyme does not exist, because this word, which applies to a ferment, cannot be applied to lecithin, which is not one and whose name is sufficient to designate it.

The new notion that is added to my determinations of 1926 is that the fibrinferment is constituted by the granular elements of the organic colibacillus. The colibacillus possesses, in fact, through its granular elements, glycolytic, lipolytic, proteolytic, and other properties.

By their lipolytic property, the granules of the coli bacillus, which are indeed zymospores as I designated them in the first volume of this work, split the lecithins into glycerophosphoric acid, fatty acids, and choline. It is the entire acid group which, attaching itself to the soda soaps of fibrinogen, modifies their colloidal state as well as that of the latter and coagulates it.

I demonstrated in 1914 (76 A) that blood globulins are a protein-soda soap complex and that their solubility is linked to the latter and to their colloidal state; by gradually becoming acidic, these gradually become then totally insoluble.

It has become customary to consider that every chemical action of a fermentative nature corresponds to a special ferment. This notion is inaccurate; a bacterial culture always possesses multiple chemical and fermentative properties, although all its fermentative elements, the granular elements, have the same origin, the same shape, and therefore probably the same constitution, and differ from each other only in their size. It is therefore that the same granulation possesses multiple properties.

Bourquelot and **Gley** have noted, for example, that *Penicilium glaucum*, cultivated on **Raulin liquid**, secretes an amylase, a casease, a chymosin, an inulase, a lypase, and a trehalase. However, it **does not secrete** ferments; it develops fermentative granulations with multiple properties.

In the liver, more than 15 different ferments have been identified so far, which, in reality, do not exist separately; these different chemical actions all come back to the colibacillary granulations that the plasma carries with it.

Dastre noted the presence of rennet in the lymph of the thoracic duct. However, the fermentative elements contained in this lymph are those of the organic colibacillus, that is, of the fibrinferment, an element which causes not only the coagulation of blood, but also that of milk, by an identical process, the hydrolysis of lecithins.

Proteolytic property. The proteolytic property of the colibacillus is manifested throughout the body since the fermentative granular elements of the organic colibacillus are carried everywhere in the tissues by the blood.

It is this property, brought into the fibrin by the granulations carried by it, which caused the digestion observed by **Dastre** in saline solutions.

It is this same property that causes the so-called aseptic autolysis of a piece of tissue or organ removed aseptically and placed in an incubator at 37°C in chloroformed water. This autolysis transforms proteins and nucleoproteins into amino acids, hydrolyzes fats and lecithins, and splits them into their components.

The presence of organic colibacillus and its fermentative elements in the blood and serum now explains why, a few days after an injection of serum into the blood of an animal of another species, the proteolytic ferment discovered by **Abderhalden** forms in the blood of the latter. The ferment that thus developed is the micrococcal ferment of the organic colibacillus of the first animal injected into the blood of the second.

It is therefore a bacterial culture, that of the organic colibacillus antigen, which develops in the blood of the injected animal. Since the organic colibacillus of different animal species are, like these species themselves, specifically different, the organic colibacillus antigen exerts, in the blood of the recipient animal, a different action from that of the organic colibacillus of the latter; for the same reason, the organic colibacillus of the recipient animal exerts its proteolytic action on the albuminoid materials of the injected serum and destroys them.

The organic colibacillus antigen which has developed in the blood of the recipient animal persists there for an extremely long time since this persistence which constitutes the state of anaphilaxis can become the cause of an anaphilactic shock after many years by a second injection of serum of the same origin.

T*he variola or vaccinal virus, which is a heterogeneous virus, persists for 20 or 30 years or more in the human organism*. All the more so, can the organic colibacillus of one animal more easily persist for as long in another animal whose blood constitutes for it a culture medium very close to its natural environment.

CONCLUSIONS WHICH RESULT FROM THE FACTS PRESENTED DURING THIS CHAPTER

1. The colibacillus which always exists in the intestine of the newborn, immediately at birth and before any ingestion into the digestive tract, comes from the organic colibacillus contained in its blood and all its tissues.

2. The colibacillus always exists normally in the blood of higher animals in both micrococcal and bacillary forms, the first being the usual and constant form, the other much less frequent. It is its elements which constitute the white layer deposited above the blood corpuscles of centrifuged oxalated blood.

In the micrococcal form, the colibacillus exists:

A) In the state of isolated granules of various sizes, some basophilic, others achromatic;

B) In the form of groups of 6 to 12 of these agglomerated granules, which are the platelets falsely called hematoblasts;

C) In the form of spherical masses containing a much larger number of basophilic and achromatic granules, which are the leukocytes. These are the germinative masses of the evolving colibacillus.

Isolated acidophilic granules, or those of leukocytes, are those that have lost their basophilic chromatic matter, neutrophilic granules those that still contain some, and basophilic formations those that are loaded with it.

3. Colibacillus normally exists in connective tissue and in all animal organs; it is to its granular elements that the tissue juice owes the property of accelerating blood coagulation;

4. Since colibacillus exists in all parts of the animal organism, its presence in the tissues during the dying period and after death is due to this fact, and not, as **Pasteur** indicated

and as is generally accepted, to a cadaveric or dying infection whose starting point would be intestinal. The multiplication of colibacillus during the dying period in the bacillary form is favored by the drop in body temperature.

As soon as circulation stops, the multiplication and development of colibacillus, that is, the beginning of putrefaction and autolysis of the tissues, begin at once in all parts of the body, all containing the elements of colibacillus;

5. The micrococcal form of colibacillus is the fibrinferment or serozyme, which possesses multiple fermentative properties: proteolytic, lipolytic, glycolitic, etc. Being of a bacterial nature, the fibrinferment granules multiply actively, and it is this multiplication which, in fibrinogen solutions treated with tricalcium phosphate, causes spontaneous coagulation;

6. The evolution of the elements of colibacillus in oxalated plasma and fibrinogen solutions shows that they easily pass into the mycelial form and that, through this evolution, they give rise to certain elements of connective tissue, notably elastic fibers: it is these elements, contained in the blood extravasated into a wound, which, evolving with great rapidity, achieve the organization of the clot and its healing;

7. The granular and fermentative elements of the organic colibacillus, contained in the blood, are the origin of the active elements of digestive ferments. The blood plasma, passing through the glands, carries them with it into the digestive juices. The venous blood that has passed through a digestive gland has a fermentative power that is significantly less than that of the arterial blood entering the gland. It is therefore the organic colibacillus that provides the ferment granulations that possess the activity in the parotid and submaxillary saliva, in pepsin, pancreatic juice, intestinal juices, and bile. The specific action that the gland can exert on the properties of the active colibacillary granulations during their passage through the glandular cells remains unknown;

8. The fibrinferment or serozyme is constituted exactly like the middle part (or mittelstück) of alexin, which is also a ferment, by the same active granulations of the organic colibacillus which possess the three proteolytic, lipolytic, and amylolytic properties which are also those of trypsin.

It has recently been claimed that the alexin of the blood is the trypsin of the pancreatic juice resorbed by the intestinal mucosa. All the preceding facts show that, on the contrary, it is the trypsin which comes from the blood whose granular and fermentative elements of the colibacillus pass through the pancreas to constitute it.

The existence and the physiological functions of capital importance of the organic colibacillus, its fermentative properties, are facts which make evident the nature and origin of alexin which, in summary, is confused with the fibrinferment;

9. From the existence of colibacillus as a normal constituent of the animal organism and its constant presence in the blood and tissues, it follows that, when an animal is given an injection of blood or serum or another organic fluid, or a tissue extract, a bacterial culture of colibacillus is introduced into its organism with the albuminoid materials contained in the fluid.

This culture develops in the blood of the receiving animal and there develops the new proteolytic ferment, which was discovered by **Abderhalden**, at the same time as colibacillus bacterial elements of different forms develops; it is these elements which cause

the urticaria eruptions and the various disorders which follow injections of serum and which constitute serum sickness at the same time as they cause sensitization and the state of anaphylaxis;

10. Anaphylactic shock, caused by a second injection of the antigen serum, results from the agglutination of the antigen colibacillary elements developed in the blood of the recipient and the arrest in the capillaries of the agglutinated clusters, resulting in a considerable slowing of blood circulation, which leads to hypotension, hypothermia, the cessation of oxidation, and death.

MECHANISM AND SIGNIFICANCE OF BLOOD COAGULATION: A NORMAL AND CONTINUOUS PHYSIOLOGICAL PHENOMENON IN THE ANIMAL ORGANISM

In the first volume of this work, the mechanism of blood coagulation has already been determined and the physiological significance of the phenomenon established. However, the determination of the nature and origin of the fibrinferment was lacking.

The facts previously presented, which establish that the latter is constituted by the micro coccal elements of the organic colibacillus and the normal physiological functions of the latter, further demonstrate the nature and origin of platelets or hematoblasts and leukocytes, masses of agglomeration of the micrococcal elements of this same colibacillus and possessing, like the fibrinferment, a coagulating power.

These new notions, as well as other acquisitions, therefore required the following revision of the original study, which also required certain clarifications.

In this study, I demonstrated that blood coagulation is caused by the progressive fixation of an acidic body to phthalein on the sodium hydroxide of the fibrinogen-soda soap complex, thus progressively decreasing their solubility and finally leading to coagulation of the complex.

As a result of this fixation, the acidity to phthalein gradually decreases in the blood or plasma and, correlatively, these liquids become more and more viscous, until they completely solidify, a point which coincides with the maximum decrease in acidity.

Beyond this point, the acidity to phthalein increases instead of decreasing, which demonstrates that the acidic body continues to form; it continues to attach itself to the coagulated fibrin, which has the effect of causing the clot to retract and the expulsion of the serum it contains.

* * *

The first notions on the mechanism of coagulation are due to **Morawitz** who, from the coagulating action of tissue juice, concluded that thrombin was formed by the union of a body existing in the tissues with a body contained in the blood. **Fuld** and **Spiro**, confirming this conclusion, called the first of these bodies cytozyme and the second plasmozyme.

Later, **Bordet** and **Delange** made known that these two bodies exist in the blood and called the plasmozyme of **Fuld** and **Spiro** serozyme.

In a communication to the Biological Society (6 bis, 1919), **Bordet** added the following conclusions to **Morawitz's** conclusion:

Bordet *and* ***Delange*** *showed that it is true even when considering the coagulation of pure blood, that is, preserved from any contamination by tissue juice; in this case, it is the blood cells, and particularly the platelets, which provide one of the generators (cytozyme), which is also found in the tissues.* ***Bordet*** *and* ***Delange*** *further noted that this cytozyme is in reality a lipoid very similar to lecithin and that it can be extracted with alcohol either from platelets or from tissue cells, muscles for example; the other generator, present in the blood liquid, received from its authors the name serozyme. By uniting, the serozyme and the cytozyme give thrombin.*

A serum very rich in serozyme is obtained by coagulating oxalated plasma with addition of CaCl2, and freed by vigorous centrifugation of most of its platelets, that is, of a large part of its cytozyme. Thrombin appears in abundance in such a serum when a trace of the lipoid (cytozyme) is added. A trace of tricalcium phosphate is sufficient to absorb the existing cytozyme, either in the serum or in the original oxalated plasma; the latter consequently loses the power to coagulate upon the addition of CaCl2 and cytozyme (platelets, tissue juices, or lipoid extracted with alcohol). But coagulability is restored if the serozyme is given back to it; it is sufficient to redissolve, by bubbling CO2, the tricalcium phosphate that had absorbed the serozyme.

On the other hand, in a second note (6 ter) **Bordet,** after having explained how, by adding tricalcium phosphate, followed by centrifugation, a fibrinogen solution is made stable, writes the following:

What is the nature of the principle absorbable by phosphate and which promotes the spontaneous insolubilization of fibrinogen removed from the plasma? I do not have the impression that it is thrombin or its generators: cytozyme and proserozyme. The addition of cytozyme to the fibrinogen solution does not significantly hasten its precipitation; an emulsion of carefully washed platelets which does not contain serozyme or proserozyme, on the contrary, produces this effect.

This last observation is exactly the opposite of the previous one, where it is clearly stated that centrifuged oxalated plasma, with a trace of tricalcium phosphate added to absorb the remaining cytozyme, loses the power to coagulate upon the addition of Ca-Cl and cytozyme (platelets, tissue juice, or lipoid extracted with alcohol).

Thus, since **Bordet** indicated that cytozyme has no effect on the coagulation of a fibrinogen solution, but that platelets accelerate it, it must be deduced that he no longer considered them to be cytozyme, or at least that they do not constitute it, as he stated. One then wonders where the cytozyme used came from, since **Bordet** did not provide any information on this point, and since tissue juice, which accelerates coagulation, should be considered serozyme because it contains its granules.

These facts, which appear obscure and difficult to explain, in particular the retarding action of tricalcium phosphate on coagulation, are very easily explained by the following facts demonstrated in the previous chapter:

1. Thrombin, the body that directly coagulates fibrinogen, is the acid body released by the action of serozyme on cytozyme, but not these two bodies themselves.

2. Serozyme is made up of granules of 0.2 to 0.7 microns that the fibrinogen precipitate carries with it and which are micrococcal elements of the organic colibacillus;

3. Tricalcium phosphate acts by carrying with it a part of the serozyme granules;

4. The platelets that **Bordet** considers to be cytozyme are groups of 6 to 12 serozyme granules; they have nothing in common with the formation of erythrocytes and are not hematoblasts; they are therefore a part of the serozyme.

Thus, it is proven that the cytozyme does not have the constitution indicated by **Bordet**; it is not constituted by platelets nor by a tissue juice; the platelets are the serozyme and it is consequently impossible to rid them of it as **Bordet** believed; what probably made him believe that the platelets were a body different from the serozyme is that the quantity that he emulsified was infinitely richer in active granulations of ferment than the

serum that he considered to be this serozyme alone; by adding this emulsion of platelets, which is a very rich serozyme, to serum, a poor serozyme, he therefore strongly reinforced the action of the latter; it is this reinforcement that could have given the appearance of two different bodies.

In any case, another fact requires explanation. A solution of fibrinogen, purified with tricalcium phosphate, coagulates spontaneously after a certain time, without prior recalcification, which confirms that lime is by no means essential to the phenomenon.

In such a solution, there exists only NaCl, fibrinogen, and the bodies that it may have carried with it in its precipitation. We know one of these bodies: serozyme granules, that is, the spores and zymospores of the organic colibacillus. Coagulation can therefore only be caused by a direct action on fibrinogen or on another substance that it may have carried with it.

Indeed, plasma lecithin is precipitated at the same time as fibrinogen. It is known that lecithin solutions are precipitated by a saline concentration slightly higher than that of body fluids. However, fibrinogen is precipitated from plasma by adding its volume of a saturated NaCl solution. Lecithin is therefore precipitated at the same time and carried along with it during centrifugation or filtration.

The fact that the serum of spontaneously coagulated blood still contains more than 1 percent of lecithin does not mean that it is not carried along with the fibrinogen precipitate, because the saline balance of the blood is not modified during normal coagulation due solely to the fixation on the fibrinogen-soda soap complex of the acid group released by the hydrolysis of lecithin; the unhydrolyzed lecithin remains in colloidal solution in the serum lodged in the empty spaces of the clot meshes from where it is then expelled by the retraction of the clot.

Fibrinogen therefore carries with it, in its precipitation, both the granulations of serozyme and lecithin; the latter redissolves with fibrinogen in 7.5% saline. Under these conditions, lecithin is hydrolyzed by serozyme as in plasma and split into its constituents: glycerophosphoric acid, fatty acids, and choline. The formation of these acids therefore explains the spontaneous coagulation of fibrinogen which is a globulin.
Indeed, I demonstrated, in 1914 (76 A and C):

1. That blood globulins are complexes formed by the bonding of a protein material with soda soaps. In the solution of such a complex, a certain proportion of soap is dissociated into free soda ions and free acid ions;

2. That if the free sodium ions are gradually neutralized by an acid, the soap of the complex becomes more and more acidic and less and less soluble until complete insolubility results in the precipitation of the entire complex;

3. That the precipitation of globulins by saline solutions is achieved by an identical phenomenon: the progressive increase in saline concentration correlatively increases the dissociation of the soap until its total insolubilization results in the precipitation of the complex.

Therefore, in a fibrinogen solution, the soaps are gradually made acidic, then insoluble by the progressive release of glycerophosphoric acids and fatty acids resulting from the hydrolysis of lecithin, which results in the coagulation of the complex.

* * *

Blood coagulation is therefore indeed caused, as **Morawitz,** then **Fuld** and **Spiro,** had concluded, by two different substances, but it is not true, as this conclusion indicated, that one of them, called cytozyme by **Fuld** and **Spiro**, comes from the tissues, this because a tissue extract added to serum exuded from the clot increases the quantity of unused thrombin it still contained.
Indeed, this cytozyme, which is thermostable, is a lecithin that is always present in the blood, just as the other body, the plasmozyme of **Fuld** and **Spiro**, called serozyme by **Bordet**, is also always contained in the blood; the two constituents of thrombin are therefore always present in the blood and do not need a supply from the tissues.

But two facts have complicated the problem, and have made certain experimental results difficult to explain; the first is that the blood serozyme, consisting of the fermentative granulations of the organic colibacillus, is also contained in the tissue juice, for example in the connective tissue and the lymph where its granulations are numerous and that it is to this presence, at least in large part, that the tissue juice owes the power to increase the quantity of thrombin when it is added to serum.

The second is that the blood platelets, which are actually groups of serozyme granules and the most active part of it, have been considered cytozymes. This error was made because, according to **Bordet**, a lipoid of the lecithin group can be extracted from platelets using alcohol and other solvents, which, when added to blood, increases the quantity already contained therein. From this second fact, it follows that an emulsion of normal platelets in salt water, added to serum, considerably increases its coagulating power because it increases the proportion of serozyme in it, and that an alcohol extract of platelets, although much less active, can also exert an action, according to **Bordet** and **Delange**, by acting as a cytozyme.

It is therefore necessary to make known, in such experiments, the exact nature of the cytozyme used and to specify precisely whether it is an alcoholic extract of platelets or tissues and whether this extract has been deprived of serozyme granules or whether an emulsion of normal platelets in salt water has been used.

Let us also remember that thrombin, the body that directly causes blood clotting, cannot directly be the serozyme-lecithin mixture (cytozyme), which is incapable of causing it, and that it is only the acid group of lecithin released by the hydrolysis carried out by the serozyme. It is this acid group which, acting on the sodium soaps of the fibrinogen-sodium soap complex, makes them acidic and insoluble and causes the complex to coagulate.

Any other organic acid contained in the blood, notably the lactic acid which normally forms there, can contribute to the same coagulation by the same mechanism.

Note here that the coagulation of casein occurs by an identical phenomenon, the lab ferment being the colibacillary diastase carried along with the milk, a liquid that contains lactose, casein, and lecithin.
The hydrolysis of the lecithin releases the glycerophosphoric acid and fatty acids, and, on the other hand, the action of the ferment on the lactose produces lactic acid.

These are the substances that make milk sour, that is, strongly acidic, when left to itself and cause the casein to coagulate, an indisputable fact since, to prevent coagulation, it is sufficient to add to the milk a sufficient quantity of an alkaline substance, such as bicarbonate of soda, to neutralize the acids as they form; the milk remains incoagulable if enough is added.

This proves that, like fibrinogen, casein is a complex consisting of a nucleoprotein bound to alkaline soaps on which its colloidal state depends and which, by binding the acids formed, becomes insoluble and causes the complex to coagulate.

Physiological significance of the phenomenon of blood clotting

It is quite obvious that it is not the fact of the blood leaving the vessels that causes the progressive decrease in phthalein acidity, then coagulation, and finally a subsequent increase in this acidity and the retraction of the clot. This is a phenomenon that occurs continuously in circulating blood and which, as occurs in vitro, results in its coagulation. But this only occurs upon contact with the anatomical elements on which the formed fibrin is deposited.

It is known that, in oxalated plasma, the addition of an equal volume of saturated NaCl solution precipitates fibrinogen and that the addition of increasing quantities of salt to the filtrate determines the progressive precipitation of other globulins, these having the same constitution as fibrinogen.

From this, it must be concluded that the only difference separating the other globulins from fibrinogen is that the soap in the latter is more acidic; it thus appears that as fibrinogen fixes itself to the tissues in the fibrin state, its proportion in the blood is kept fixed by the passage to the fibrinogen state of the most acidic and most easily precipitated globulin; this passage is achieved by the fixation of the acid portion of the hydrolyzed lecithins on the free sodium ions of the plasma, a fixation which leads to a progressive increase in the dissociation of the soap from the globulins and a growing decrease in their solubility.

This role of the phenomenon of blood coagulation is made evident by the known fact that the coagulation of venous blood produces less fibrin than that of arterial blood. This fixation of globulin on the anatomical elements, which constitutes the phenomenon of the incessant renovation of living matter, must also obviously occur for other types of protein materials, nucleoproteins, glycoproteins, and I add cholesteroprotein, because there is a type of protein linked to cholesterin soaps, a complex which, like the protein-soda soap complex (globulins), is precipitated by salts, that is, rendered insoluble by progressively increasing its dissociation in solutions.

These various protein substances must probably, like fibrinogen, undergo a precipitation which deposits them on the anatomical elements as they gradually wear down due to their physiological functioning.

Several facts have contributed to masking the physiological significance of the phenomenon of blood coagulation; among them, first and foremost, are the absence of coagulation of blood from a vein in the space between two ligatures and the delay or absence of coagulation of blood received in a vessel with paraffin or oiled walls. These are exceptional cases, the study of which is not sufficient to draw general conclusions; in particular, they lack knowledge of what happens to the acidity of blood with phthalein and its visquosity, notions essential for judging their significance.

However, inaccurate conclusions have been drawn from this phenomenon; it has been concluded, for example, that fibrinferment does not exist in circulating blood and that it only appears after it leaves the vessels. This is inaccurate since this ferment is made up of serozyme granules and platelets, which are groups of the same granules, objects of constant presence in the blood; being formed elements, it is impossible for them to appear only at the moment when they leave the vessels. They are also active there since

the blood coagulates rapidly in the cavities of the heart and the large arterial vessels very soon after the total cessation of circulation.

On the other hand, the presence of fibrinferment or serozyme in the blood cannot be questioned since it is seen there. Its action cannot be doubted since by removing a good portion of these granulations from the oxalated plasma by the action of a precipitate such as gelatinous tricalcium phosphate, its spontaneous coagulation is prevented for a long time. Moreover, there is certainty that it acts inside the vessels, since arterial blood yields to the anatomical elements of fibrin that are no longer found in venous blood.

Finally, the phenomenon of lecithin hydrolysis in blood, plasma, and serum is established with certainty by experiments by **Doyon** and **Morel** (14 bis). These authors determined that serum collected after coagulation of dog blood after fasting for 24 hours and originally containing 3 grams per 1,000 of organic acids fixed in the state of ethers, contains only 0.77 grams after having remained in an oven for 144 hours at 370; but that if this serum is centrifuged from the start, it still contains 2.78 grams after 144 hours in the oven, which proves that centrifugation has had the effect of making the phenomenon approximately 8 times less active. Correlatively, the decrease is accompanied by a release of 0.9 grams of organic acids combined in the state of soaps or free.

Now, the acids combined in the ether state are, in plasma and serum, almost exclusively lecithin and cholesterol ethers, oleates and palmitates. There is therefore hydrolysis of lecithin. On the other hand, centrifugation of serum has the effect of considerably reducing this hydrolysis because it removes a large part of the hydrolyzing formed elements, which are those of serozyme.

There can therefore be no doubt about the reality of the hydrolysis of lecithins and the release of the acids that constitute them.

The intensity of the phenomenon should not be judged by the quantities of lecithin that the serum or blood normally contain, because they represent only an equilibrium proportion; the intensity of the phenomenon could only be known by measuring the coefficient of blood irrigation of a region in the unit of time, combined with the measurement of the loss of lecithin that the arterial blood undergoes while crossing it.

CONCLUSIONS

1. Plasma oxalated at 1.5 or 2% 1,000 is acidic to phenolphthalein; from the moment of its recalcification, this acidity decreases to a minimum which coincides with the precise moment of total coagulation; from this moment, on the contrary, it increases progressively;

2. Coagulation results from the action of the acid body formed on the fibrinogen-soda soap complex; making the latter more and more acidic, it progressively insolubilizes them, as well as the entire complex;

3. The formation of the acid body results from the hydrolyzing action of the fibrinferment on the lecithin of the blood, which it splits into its components: glycerophosphoric acid, fatty acids, and choline. It is therefore a group of acid bodies which is formed and not just one;

4. The body called cytozyme by **Fuld** and **Spiro** and which would be supplied by the tissues is the lecithin of the blood; the name cytozyme is therefore inappropriate for its constitution and it is preferable to keep the name lecithin;

5. The body called plasmozyme by **Fuld** and **Spiro** and serozyme by **Bordet** and **Delange** is the fibrinferment itself and this is made up of granules of 0.2 to 0.7 microns or more which are the micrococcal and fermentative elements of the organic colibacillus having proteolytic, lipolytic, glycolytic, amylolytic, etc. properties;

6. The granular elements of the colibacillus are presented either in the form of scattered and isolated granules, or in the form of agglomerations of 6 to 12 granules (platelets or hematoblasts) or a much larger number, united in a mass spherical (leukocytes);

7. Thrombin is the group of directly coagulating acidic bodies and not serozyme or fibrinferment, nor cytozyme or lecithin, nor the mixture of these two bodies; it is the result of the action of the first on the second;

8. The gelatinous tricalcium phosphate precipitate acts on oxalated plasma and on fibrinogen solutions like a net that captures and carries away a certain quantity of fibrinferment or serozyme granulations by centrifugation. It rarefies them, but does not carry them all away; on the other hand, it leaves intact the lecithin that is in solution;

9. The action of tricalcium phosphate on oxalated plasma or on fibrinogen solutions delays their spontaneous coagulation, but does not prevent it;

10. Serozyme or fibrinferment does not exist in the state of proserozyme. It is only because it is greatly rarefied in a solution of fibrinogen treated with tricalcium phosphate that its action is delayed and it is because, being of a bacterial nature, it multiplies considerably and manages to exist in sufficient quantity to be able to hydrolyze lecithin and release the coagulating acid bodies. It is therefore in no way a transformation of proserozyme, but only a multiplication;

11. The presence or absence of lime salts has no effect on the phenomenon of coagulation; the addition of calcium chloride to oxalated plasma only acts by insolubilizing sodium oxalate and thus suppressing its agglutinating action, the only cause of the incoagulability of plasma being its presence and not by the precipitation of lime salts;

12. Hirudin makes the blood incoagulable by agglutinating the active elements of the fibrin ferment (organic colibacillus), particularly the platelets, a state which makes it impossible for them to exert their hydrolyzing action on the lecithin and to multiply.

Calcium oxalate and anticoagulant salts act by agglutinating and rendering inactive the active elements of the fibrin ferment, particularly the platelets.

13. Staphylococcus, a form derived from the normal micrococcal state of the organic colibacillus, whose fermentative and coagulant properties it has retained, is identical to fibrinferment (serozyme) and acts by the same mechanism as it by hydrolyzing the lecithin in the blood and releasing its coagulating acid group, which is thrombin;

14. The phenomenon of coagulation of blood leaving the vessels is not a cadaveric phenomenon, but a normal physiological phenomenon, continuous in the circulating blood, and whose result is the fixation of fibrin on the anatomical elements and the repair of the losses they undergo due to the physiological work they perform. It is the continuous phenomenon of the maintenance and renovation of living matter. It is one of the most important functions performed by the organic colibacillus, a fundamental and indis pensable constituent element of animal organisms.

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CHAPTER IX

THE DIFFERENT FORMS TAKEN BY THE ORGANIC COLIBACILLUS IN THE ANIMAL BODY

These forms include:

1. Staphylococci;

2. Streptococci;

3. Pneumococcus;

4. Enterococcus;

5. Tetragenes;

6. Pneumobacillus;

7. *Bacillus lactis aerogenes*;

8. Pasteur's septic vibrio and Legros' aerobic septic bacillus;

9. Tetanus bacillus, studied in Chapter XII;

10. Ozena and Rhinoscleroma bacilli;

We will successively study these various forms, all of which derive from the organic colibacillus or fibrinferment (serozyme) and have retained, more or less, some of its chemical properties, notably that of clotting blood and milk.

PYOGENIC STAPHYLOCOCCUS

The staphylococcus found in pus, abscesses, osteomyelitis, and boils is the micrococcal granulation of E. coli, which exists in considerable quantities in connective tissue and in the blood.

In its in vitro cultures, it affects the three forms: aureus, citreus, and albus, which, for bacteriologists, differ only in color, their biological properties being identical.

Morphological characteristics. It appears in the form of spherical cocci 0.4 to 1 micron in diameter, isolated or grouped in groups of two grains, three or four, and even five or six in chains or numerous irregular clusters. A chain of four grains means that it comes from the segmentation into four parts (two by two) of a bacillus that was the length of the chain. It takes the Gram.

Biochemical characteristics. - It peptonizes egg white, that is, it possesses the proteolytic power; it transforms lactose into lactic acid; it produces acetic, valerianic, butyric, and propionic acids; it produces indole. It coagulates oxalated blood plasma and fibrinogen solutions, which means it possesses lipolytic power, and it hydrolyzes lecithins.

From a study on blood coagulation by staphylococcus, **André Gratia** (26) concluded that it has exactly the same properties as fibrinferment and that it acts like fully formed thrombin.

Its culture appears more heat-resistant than fibrinferment and alexin mittelstück, which are inactivated at 58°C after 30 minutes, but it is nevertheless killed by exposure for 24 hours at 55°C and 15 minutes at 80°C.

It therefore exhibits exactly the same characteristics as colibacillus, of which it is the micrococcal form. Since the latter exists everywhere, in the blood and in all parts of the animal body, it is therefore not surprising that staphylococcus is also found everywhere, on the skin, mucous membranes, and in all parts of the digestive tract.

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I recall that, in the first volume, it is indicated, page 655, that I was able to transform *Staphylococcus pyogenes aureus*, of human origin, by culture on solid medium, into *Penicilium* form (fig. 3, 4, 5 pl. 307 first volume) and *Aspergillus* (fig. 1, 2 pl. 307 first volume) and that these forms having appeared identical to those of the organic mold of man had led me to conclude that furunculosis is an autogenous disease of man. The new knowledge of the existence of the organic colibacillus and its functions, the identification of the active micrococcal granulations of the fibrinferment and that of the tissue juices with the micrococcal form of the organic colibacillus now completes the previous determination by showing that the staphylococcus, which is the micrococcal form of the organic colibacillus, normally exists in the connective tissue, in the blood and in all tissues.

Staphylococcal lesions, phlegmons, abscesses, osteomyelitis, etc. therefore have an autogenous origin and development and are not due to a heterogeneous infection whose starting point would be the surface of the skin and the cavity of the digestive tract.

It seems certain that staphylococcus is the colibacillary micrococcus of connective tissue that has undergone slight adulteration, which, however, has not caused it to lose the properties of normal colibacillary cocci (fibrinferment), notably that of grouping into germinative masses which are leukocytes and pus globules. The pus leukocytes are

therefore those of the staphylococcus culture emerging on site and are not blood leukocytes that have come there to play a phagocytic role.

What has been called phagocytosis is the normal mode of formation of leukocytes. Instead of playing a phagocytic role, they are, on the contrary, a phase in the multiplication of colibacillary elements. This explains why it has been clearly seen that staphylococcus or streptococcus grains are contained within leukocytes.

The multiplication of staphylococcus occurs by the formation of filaments emanating from leukocytes and which segment into bacilli, which themselves segment into granules. Formal proof of this fact is the existence of diplococci and short chains of 3, 4, and even 5 or 6 cocci.

The relationship between leukocytes and the cocci forms of colibacillus (staphylococcus, streptococcus, pneumococcus) is highlighted, in a very interesting way, by **Le Fèvre de Arric**, in two notes (38) on the rate of mononuclear cells in the secretions of war wounds treated with **Carrel's** liquid; **Le Fèvre de Arric** notes, after the latter, that the number of mononuclear cells increases progressively as the number of bacterial germs decreases This is how he found that the average rate of mononuclear cells per 100 cells, which is only 0 to 2% when the number of germs is extremely large and difficult to measure, rises to 6% when it decreases to around 20 germs per microscopic field of a preparation of the exudate, then to 10% when it decreases from 20 to 2, and finally to 17% when the germs tend to disappear (1 to 0).

The author does not explain this result, which does not mean that the germs are destroyed, but only that the exudate undergoes an evolution during which these germs agglomerate to pass into the state of germinative masses which are the mononuclear cells and that these masses no longer evolve rapidly, that is to say no longer undergoing the bacterial evolution in four stages, accumulate in the exudate, which explains the increase in their number in quantities inversely proportional to the number of bacterial germs.

All this is explained by the fact that the germs considered as infective agents of war wounds (staphylococci, streptococci, etc.), are adulterated elements of the normal organic colibacillus of the subject whose pus leukocytes are germinative masses; the most perfect state of normal leukocytes, their state of maturation is the mononuclear cell whose nucleus occupies the entire mass.

The bacterial germs of war wounds are no different from those of suppurations after the most aseptic surgical operations.

* * *

A study by **Mosny** and **Marcado** (6, p. 550) on the effects of injecting animals with staphylococcal toxin is of great interest for demonstrating the relationships that exist between colibacillus and staphylococci. According to these authors, the injection of staphylococcal toxin causes the development of intraintestinal abscesses and colibacillus purulent peritonitis because they cause the passage of intestinal Bacterium coli through the mucous membrane.

In all observations of this kind, the interpretation of the facts is always given in such a way as to agree with pasteurian dogmas. Here, the interpretation is false:

1. Because staphylococcus and colibacillus are the same microorganism with multiple forms;

2. Because a toxin is made up exclusively of the smallest formed elements of a culture, elements which pass through the filters;

3. Because the formed elements of a toxin are capable of completely reproducing, even in vitro, all the elements of the original culture.

What **Mosny** and **Marcado** injected into their animals were therefore the smallest figurative elements of a staphylococcus culture, elements of the same nature as the large ones, capable of reproducing them; their experiment gave them the only result that could result from it, the reproduction of the colibacillus of which the staphylococcus is only one form. Their experiment proves:

1. That the staphylococcus is a form of *Bacterium coli*;

2. That the culture of a bacterium can include both granular elements (cocci) and bacillary elements, without this coexistence of two forms constituting a microbial association;

3. That monomorphism is one of the biggest errors of dogmatic bacteriology.

After this fact, let us cite that of **Charrin** and **Veillon,** who, noting the existence of pneumococcus in a peritoneal effusion of a living subject, found it disappeared at autopsy and replaced by colibacillus. The pneumococcus had evolved under the very probable influence of the lowering of body temperature, and had resumed the bacillary form of colibacillus (see "*Agonal Infections*," p. 247).

Let us also cite the fact observed by **Le Fèvre de Arric** who, separately isolating colibacillus and streptococcus from a wound, could only find colibacillus when he inoculated them together in broth. Only one reappears, colibacillus, because they are both colibacillus and have a common germinative mass

Action of staphylococcus on blood coagulation.

Much (51) and **Kleinmich** (36) observed that staphylococcus, added to oxalated plasma, causes it to coagulate after several hours in an incubator at 37°C. Staphylococcus possesses this property because, like fibrinferment, it is a micrococcal form of the organic colibacillus that has retained its fundamental fermentative properties; the same is true for the pneumococcal and enterococcal forms (see below).

This origin of staphylococcus implies that it exerts its coagulating action by exactly the same mechanism as fibrinferment, that is, it acts by hydrolyzing the lecithin in plasma. It is therefore not through a staphylocoagulase that this action is exerted, as **André Gratia** believed. In short, staphylococci, streptococci, and pneumococci, all endowed with the same coagulating property, are nothing other than fibrinferment, either in its entirety or in a form superficially modified in a transient and slight manner.

Gratia (26 C and 26 D) indicates that, in oxalated plasma with tiny traces of staphylococcus added, coagulation begins as soon as the culture has grown sufficiently. This shows that the staphylococcus behaves exactly like the so-called proserozyme; using the same terms, we can say that the traces of staphylococcus inoculated are prostaphylococcus.

Gratia further states that the coagulating action of staphylococcus does not require the two thrombin-producing elements because it is exerted equally well on oxalated phosphated plasma and on a phosphated fibrinogen solution, which, he writes, no longer contain the two thrombin generators. This statement is incorrect; the addition of tricalcium phosphate to oxalated plasma and fibrinogen solutions does not completely deprive them of the two thrombin generators; although in lesser quantities, they still contain the two generators, serozyme and blood lecithin, that is, the two elements necessary for the liberation of the acid group of the latter by the hydrolysis it undergoes through the action of serozyme. True thrombin is this acid group of lecithin, and not the serozyme-lecithin pair of the two generators that determines its release.

It is because a fibrinogen solution always contains these two generators that it always ends up coagulating spontaneously when the rarefied serozyme, which is, like staphylococcus, the granular form of the organic colibacillus, has been able to multiply sufficiently.

In **André Gratia's** experiments, staphylococcus behaves exactly like serozyme (which is the fibrinferment), with which it merges, and its addition to phosphate plasma or fibrinogen solutions has no other effect than to reinforce its action.

In the first volume of this work, I indicated (p. 657) that I had given up searching for the original source of *Streptococcus pyogenes* because I had noticed that, in many bacterial cultures whose original source was determined, elements could appear indifferently in the form of chains or as isolated elements and clusters of irregular shapes, which meant that the chain shape is not a specific characteristic.

J. Courmont had already clearly drawn this conclusion in his *Précis de bactériologie* (p. 297). In his *Précis de microbiologie clinique,* **F. Bezancon** writes:

In pus, the streptococcus appears as immobile cocci, generally rounded, grouped end to end in diplococci, and more often, in short chains of 4, 6, 8, 10 elements.

This conception of the chain is not exact; I have already explained, in 1926, (p. 657, first volume) that this arrangement does not exist because the elements were grouped end to end, but only because they were formed by the segmentation of a long mycelial filament which segmented, first into bacilli much longer than wide and therefore each of these in turn segmented two or three times in succession until it reached the granular form. These elements remain associated in chains because the achromatic matter which constituted the framework of the original filament persists, at least in part, and keeps the elements united together as long as it has not dissolved in the liquid.

In the first volume, I gave as proof of the accuracy of this explanation that we frequently observe chains of cocci in which several bacilli have not yet segmented and even chains in which a long part of the mycelial filament is still undivided. There are also chains of diplococci, in which these are the witness of the last segmentation. Fig. 392 of **J. Courmont's** *Précis de bactériologie*, containing the various aspects of streptococcus pyogenes, shows, in box C, chains of rectangular elements that the author compares to streptobacilli and which are, in reality, chains of elements that have not yet completed their last segmentation.

Let us also point out that box B of this same figure shows a staphylococcal arrangement from which chains originate.

From these observations, we can conclude that, in short, streptococcus does not possess a clear morphological characteristic that allows it to be specifically differentiated from staphylococcus.

The mechanism of chain formation having been explained, let us seek its origin. It results from the formation of a mycelial filament which, always, inevitably, itself arises from a germinative mass that develops it by germination.

Pyogenic streptococcus forms abscesses and phlegmons whose pus contains a considerable number of leukocytes, which are precisely the germinative masses that form the mycelial filaments that become streptococcal chains.

These leukocytes, germinative masses, are formed by the elements of the streptococcus, agglomerated together. In his *Précis de microbiologie clinique*, **Bezançon** indicates (p.174) that: "*these micrococci are free or included in the protoplasm of the leukocytes*." They are not only included there; they constitute the entire leukocyte, exactly as the staphylococcus constitutes the entirety of the leukocytes in staphylococcal pus, a coincidence that is

not surprising, streptococci and staphylococci being only different forms or even aspects of the same object, the organic colibacillus, which itself presents itself in a third form, the bacillary form, and can also present itself in several others (pneumococcus, enterococcus, etc.).

This extreme polymorphism of the colibacillus is well known. It is certainly due to the fact that the various elements of the organic colibacillus exist in all parts of the body, and consequently, in environments of very different constitutions. The blood environment is different from that of the loose, subcutaneous connective tissue, or from that of the intermuscular lamellar connective tissue, from that of the liver, the spleen, the pleural, pericardial, or peritoneal cavities, etc. It is obviously these differences in environments that determine the different appearances and slight differences in the properties of the organic colibacillus, depending on the parts of the body from which the culture emanates, a difference such as, for example, the macroscopic appearance of staphylococcus and streptococcus cultures, or such as that of the pneumococcus, which possesses its shell and its classic appearance when cultivated in serum or in a medium containing it and immediately loses it if transferred to broth no longer containing the albuminous elements of blood.

The proof that colibacillus, staphylococcus, streptococcus, and pneumococcus are the same object, appearing in different and often even similar forms, results from all the facts presented here in the articles concerning them and also from those which follow:

We read, in the *Précis de microbiologie clinique* by **F. Bezancon** (p. 172):

Streptococcus is the agent of erysipelas; it alone seems capable of producing this lesion. ***Achalme****, in 43 cases of erysipelas, was unable to isolate other microorganisms.*

We also note on page 178:

The production of an erysipelas plaque is not the exclusive preserve of streptococcus; experimental erysipelas can be reproduced with pneumococcus, and also, according to ***Nicole****, with Bacterium coli and Staphylococcus.*

There is therefore an identity of pathological action of these four forms of colibacillus. Here is now another demonstrative fact.

A note by **Le Fèvre de Arric** (39) on the germinative properties of wound streptococci contains the following passage:

We would like to point out in passing that, with regard to associations, colibacilli can be of some interest. Several times, we have encountered colibacilli and streptococcus isolated on agar, while the bacillus grew alone in a liquid medium (broth or even serum broth). Simultaneous inoculation in the same medium of the two purely isolated germs reproduced the same phenomenon; this proves once again that searching for streptococcus by a single culture is absolutely insufficient.

The author has not explained the phenomenon he observed, but he must have thought that in the company of colibacillus, streptococcus does not grow in broth. Here is the real explanation of the fact:

The two forms, streptococcus and colibacillus, inoculated separately, reproduce without change, because, although unstable, no influence intervenes to modify them. Being inoculated together in the same broth, the conditions are no longer the same. Belonging to the same microorganism, the granular and bacillary elements agglomerate indiscriminately to form germinative masses which will give rise to mycelial filaments which will evolve towards the highest form, which is the bacillary form, a form which differs from the streptococcus form only in that segmentation is not pushed to the micrococcus state, and in that the elements separate completely.

This fact is similar to the observation of **Mosny** and **Marcado**, who, injecting staphylococcal toxin, harvested colibacilli, and to the observation of **Charrin** and **Veillon**, who, isolating pneumococcus from a peritoneal effusion, noted at autopsy its disappearance and its replacement by colibacillus, into which it had been transformed by lowering the temperature.

The identity between streptococcus and colibacillus results, on the other hand, from the identity of their properties. Indeed, like colibacillus, streptococcus coagulates milk and oxalated blood plasma; it is killed by exposure for one hour at 52°C (**Besson**) and 30 min at 57-58°C, like fibrinferment or serozyme, which is the micrococcal form of colibacillus in the blood; like that of colibacillus, these cultures become acidic because it produces acids, notably lactic acid at the expense of sugars.

It does not produce indole, but this fact is insufficient to separate it from colibacillus, the properties of the latter and of all the forms and varieties it presents being, as we know, extremely variable, not only depending on the culture medium, the original source, and its age, but from one generation to the next of the same strain.

To study the coagulant action of streptococcus on milk or blood plasma, it is not appropriate to use a strain of unknown origin, but exclusively a strain originating from the animal that provided the milk or blood.

According to **Gratia** (26 A, pl. 246):

"The action of the hemolytic streptococcus is extremely variable; sometimes it increases the coagulability of oxalated plasma, sometimes, on the contrary, it decreases it; most often, it makes the oxalated plasma incoagulable after a few hours. Like staphylococcus, it would reduce the stability of fibrinogen, then flocculate it partially (and, in fact, we see the streptococcus clump at a certain point) and then completely. We would therefore have, depending on the stage of the process, first a more coagulable plasma, then a less coagulable plasma because it is partially defibrinated, and finally, an incoagulable plasma because it is totally defibrinated..."

Gratia adds that the oxalated plasma made incoagulable by the streptococcus has the power to notably delay the coagulation of the normal recalcified oxalated plasma and to completely prevent the coagulation of the oxalated plasma by the fibrinferment. And he concludes from these facts that the "*plasma of streptococci*" is incoagulable because it contains large quantities of antagonistic substances.

What **Gratia** described is indeed the coagulant action of streptococcus on blood plasma, and the lumps formed are fibrin lumps containing masses of streptococci. The more or less defibrinated plasma that remains delays or prevents the coagulation of the oxalated plasma, whether recalcified or not, because it contains streptococcus, which immediately carries with it in its development the serozyme of the plasma to which it is added. Streptococcus has a weak coagulant action because it is its isolated and free micrococcal elements that alone act, the chains being inactive.

Indeed, the aging of thrombin, and more precisely, of serozyme, that is, the loss of its activity, is due to its development into the streptococcus form and germinative masses (leukocytes).

* * *

Hirudin makes the blood incoagulable because it agglutinates the platelets, alters them (Aynaud), and prevents serozyme from exerting its hydrolyzing action on lecithin.

Sodium oxalate also acts on the blood by agglutinating the platelets. A large mass of agglutinated platelets from oxalated dog blood is visible in Figure 7, Plate 74.

* * *

There are streptococci that take gram-positive bacteria, others that do not take it or take it inconsistently; that of mastitis in dairy cows does not take it; that of gangrenous mastitis in sheep does take it. The colibacillus, from which streptococcus derives by evolution from its cocci, does not take it, while its various forms, tetanus bacillus, septic vibrio, staphylococcus, pneumococcus, etc..., do take it.

This diversity of reaction to *Gram* of the colibacilli and its forms is due to the fact that these are already more or less evolved and to the change of environment. The reaction to *Gram* is therefore not a specific characteristic and cannot be invoked to affirm that two forms are unrelated or unrelated, if one takes *Gram* and the other does not; for example, the tetanus bacillus and the colibacilli, the septic vibrio and the colibacilli.

* * *

It is explained above that a strain of streptococcus is obtained quite easily by preserving, at laboratory temperature and in a sterile tube, serum from an animal collected aseptically, notably serum from a horse; the streptococcus which thus appears is formed by evolution of the serozyme and it is this evolution which constitutes precisely what is called the aging of the serozyme or of the thrombin and which is its evolution into streptococcus.

This phenomenon is exactly the same as that of the aging of the Mittelstück or middle part of alexin or complement. Dialed against spring water, the serum, by deprivation of NaCl which makes the globulins insoluble and precipitates them, is thus separated into two parts which are isolated by centrifugation. The precipitate (Mittelstück) called the middle part of alexin is thermolabile. The liquid part, the terminal part of alexin, is thermostable; the active body of the mittelstück is the serozyme or fibrinferment which is carried along with the globulin precipitate and which is inactivated in 15 to 30 min. by a temperature of 57-58°.

If, after centrifugation, the mittelstück is left as a pellet at the bottom of the tube, it can be stored for a fairly long time without deteriorating. If it is redissolved in water with added NaCl, it deteriorates easily.

This is the **Brand** phenomenon, unexplained until now, because it was unknown that the active body of the Mittelstück is the serozyme, or fibrinferment, that the fibrinferment is

the micrococcal element of the organic colibacillus, and that this micrococcal element, which is the serozyme, evolves in the serum and the Mittelstück, transforming into streptococcus, then into colibacillus.

* * *

There is another formal demonstration of the transformation of colibacillary cocci in the blood (serozyme) into streptococcus.

I indicated in the first volume, page 245, that a dog being sensitized by one or more intravenous injections of horse serum, a few drops of serum from this dog, prepared 15 or 20 days after the first anesthetic injection, added to fresh horse serum, cause it to flocculate and that microscopic examination of the flakes that appear shows that some contain long, tangled chains of cocci, some of which, still rectangular, have not yet completed their final segmentation (fig. 5, pl. 22, 1-; vol.) while others (fig. 6, same pl.) are made up of large masses of agglutinated granules, in a state of germination; we also find, scattered in the liquid, numerous chains of cocci which are completing the final segmentation; we see one of them, in the upper left corner of figure 6 of this same plate, whose cocci are still joined two by two.

The formation of chains of cocci, that is, streptococci, can therefore be easily achieved with the blood of an animal and this formation is achieved, without there being the slightest doubt in this regard, by the evolution and multiplication of the micrococcal elements of the organic colibacillus contained in the blood, elements which are the ferment of fibrin; their evolution and multiplication in the serum which contains a little of it, requires a certain time devoted, it was believed (**Bordet**), to the transformation of a proserozyme.

Thus, contrary to dogma, there are always micrococcal granulations in the blood and these can develop chains of streptococci without the intervention of any external germ.

Let us also note that urine and milk, which carry an enormous quantity of colibacillary cocci from the blood, develop, when left to their own devices, and like blood serum itself, cultures of small chains of streptococci. It thus appears that the colibacillary cocci of the blood, which is the fibrinferment, is the source or a source of the strains of streptococci that are isolated from animal organisms.

The subcutaneous or deep connective tissue and the dermis contain the same micrococcal granules belonging, like those in the blood, to the colibacillus, and which, like them, can multiply into clusters or isolated granulations that are taken for staphylococci or into chains of cocci, which have been called streptococci. It is therefore not surprising that **Achalme** isolated streptococcus 43 times out of 43 cases of erysipelas, the seed that causes it existing at the very place where it develops.

Development of streptococcus in normal body fluids

It is stated elsewhere that the active granules that constitute the digestive ferments of the animal come from the blood from where they pass into the digestive juice by crossing the glands, known until now to produce these ferments entirely.

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All fluids that pass through the mucous or serous membranes carry with them the micrococcal colibacillary granulations from the blood.

URINE. - These granulations normally pass through the renal epithelium and are always present and easy to detect in all normal urine. They are the ones that cause the putrefaction of the urine. It is they that, evolving in the urinary ducts themselves, develop the complete culture of colibacillus and cause urinary colibacillosis. This is generally due not to a lesion of the renal epithelium, which is intact, but to special conditions in the urine that allow the abnormal evolution of normal granulations.

If normal urine is collected in a sterile tube and placed in an incubator, it quickly develops a culture in which numerous chains of cocci are found, which are streptococcus as legitimate and in the same way as that removed from erysipelas.

MILK. - Bacillus lactis aerogenes. Milk contains an innumerable quantity of granulations that come directly from the blood and are the active elements of the ferment that causes it to coagulate. colibacillus, streptococcus, staphylococcus, and pneumococcus are known to coagulate milk.

The active elements of the casein ferment are the same as those of the fibrin ferment, and they are both the micrococcal elements of the organic colibacillus. Therefore, it is not surprising that milk coagulates oxalated blood or plasma.

To see these granulations in milk, simply crush a coagulated fragment of casein, the size of a millet grain, in a drop of water, or crush it between two glass slides and examine it after staining with dahlia. The same granulations as those contained in the blood clot are visible, some emitting a short, divaricate filament bearing fine zymospores. In addition, the clot contains bacillary elements of variable length, which are those of *Bacillus lactis aerogenes*, which bacteriologists consider to be part of the colibacillus group.

Seeded in a draft, a plot of coagulated casein develops chains of micrococci which are streptococcus, just like that of erysipelas; inoculated in a 2% peptone solution in salt water, this culture where a plot of coagulated casein develops the bacillary form of *Bacillus lactis aerogenes*, which is in reality the organic *Bacterium coli* of the subject who provided the milk.

During the secretion of milk, the same phenomenon occurs that I described for the secretion of parotid saliva of the ox; the milk carries with it the fermentative granulations of the blood, which are those of the organic colibacillus.

The streptococcus, developed in broth by a fragment of curdled milk, is the same as that which **Nocard** described as the agent of contagious mastitis in cows; in both cases, it is a colibacillary mycelial filament segmented to the state of cocci and whose segmented elements remain attached; the word streptococcus has no specific meaning and only designates, in the particular case which concerns us, a chain of colibacillary cocci which have not dissociated after segmentation.

The coccus that **Nocard** demonstrated to be the agent of gangrenous mastitis in ewes is the normal colibacillary micrococcus of milk, altered in its power of multiplication; **Bridré** rightly considered it a normal host of the udder; it does not occur in chains, but as isolated cocci or in groups of two or three.

Mastitis in cows and sheep are therefore autogenous diseases accompanied by abnormal vegetation of colibacillary cocci, which is probably the cause.

Thus, in summary, the micrococcal and fermentative granulations of the blood colibacillus pass directly into the milk, and when transferred from the milk to broth, they evolve into colibacillus in the form of *Bacillus lactis aerogenes* or small chains of streptococci, just as the colibacillary granulations of the blood, obtained by centrifugation of oxalated plasma, reproduce this same colibacillus in broth, and small chains of streptococci develop spontaneously in serum.

Thrombin in the blood coagulates milk; milk coagulates blood; colibacillus, staphylococcus, streptococcus, pneumococcus, and *Bacillus lactis aerogenes* coagulate blood and milk. **Dastre** discovered a rennet (which is the colibacillary cocci) in the lymph of the thoracic duct that contains serozyme and white blood cells.

By injecting staphylococcus, colibacillus abscesses are determined. By inoculating streptococcus and colibacillus into broth, born side by side, only colibacillus develops; pneu-

mococcus transforms into colibacillus; chains of coli bacilli develop spontaneously in oxalated plasma. Streptococcus chains develop spontaneously in serum, urine, and milk stored at room temperature.

Add to these facts a whole group of properties common to all these aspects of colibacillus, and we will have a body of evidence so demonstrative that it can no longer be disputed that staphylococci, streptococci, pneumococci, enterococci, and *Bacillus lactis aerogenes* are forms of organic colibacillus, have no fixed specific character, and should not be cataloged as different species, let alone as genera.

PNEUMOCOCCUS

Pneumococcus was discovered in 1881 by **Pasteur,** who found it in the saliva of a child who had died of rabies. He later also found it in the saliva of children who had died of bronchopneumonia, and then in the saliva of healthy people. It was then observed by other observers in the saliva of healthy subjects. Netter found it 4 out of 5 times in the saliva of subjects who had already had pneumonia and, once in five, in subjects who had never had it.

It is the usual inhabitant of the oral and nasal cavities, and of tonsillar mucus. It is considered the agent of pneumonia.

Like staphylococcus and streptococcus, it causes suppurations in the most diverse parts of the body, allegedly causing pleurisy and fibrinous or purulent pericarditis, meningitis, endocarditis, arthritis, simple or purulent peritonitis, nephritis, parotitis, otitis, conjunctivitis, keratitis, etc.

Morphology. – It appears in pneumonic sputum in the form of a diplococcus formed of two barleycorn-shaped cocci enclosed in a transparent capsule that stains easily with eosin. But it also appears in the form of isolated round grains or diplococci; in pus, it often appears in chains of round grains, exactly similar to chains of streptococci.

Cultures are destroyed in 10 minutes at a temperature of 56°;
It coagulates blood, plasma, and milk.

Here are various characteristics which, along with the existence of the capsule, are very important for researching the nature and origin of the pneumococcus.

Serum, broth with added blood or serum, are culture media considerably more favorable than any other for the pneumococcus. It appears there with its typical characteristics.

In media not containing globulins, it loses its shell. In non-albuminous broth, it appears as isolated grains, most often round, or as diplococci, and in numerous chains of diplococci (*Streptococcus lanceolatus Pasteuri, Gamaléia*).

* * *

All these characteristics together establish that the pneumococcus is the ferment of fibrin or plasmozyme, serozyme, that is, the micrococcal element of the organic colibacillus, which is also the streptococcus with which it is confused.

It is because of this nature and origin that pneumococcus is normally found in the oral and nasal cavities, in tonsillar mucus, an*d especially in the saliva of healthy subjects,*

whose fermentative agent is composed, as demonstrated in my 1933 publication in the Museum's tercentenary volume (77), of the fermentative elements of the blood, passed directly into the saliva through the parotid gland, that is, by the fibrinferment.

This nature and origin of pneumococcus is, like that of streptococcus, vividly illuminated by all the following facts, already set out, and which I briefly recall:

1. Fresh, normal serum, collected aseptically and stored at laboratory temperature, flocculates after a certain time, and its flakes contain cocci, diplococci, and chains of cocci.

Blood, oxalated plasma, and fibrinogen solutions contain a considerable mass of granulations, which are the fibrinferment and which cause these liquids to coagulate by multiplying and developing, under certain conditions, long bacillary chains of *Bacterium coli*.

3. Circulating blood contains, in normal animals, bacillary elements of the organic coli bacillus, as well as micrococcal elements.

4. The addition to horse serum of a few drops of serum from a dog sensitized for 15 to 20 days by intravenous injections of horse serum causes the formation of flakes consisting of long, tangled chains of cocci and diplococci identical to those of streptococcus and pneumobacillus.

5. The fibrinferment and the fermentative middle part of alexin or complement (Mittelstück) are inactivated in 15 to 30 minutes at a temperature of 57°. 20

6. Like the fibrinferment or serozyme, the pneumococcus, the staphylococcus, and the streptococcus coagulate blood, oxalated plasma, and milk because they are all the same element: the micrococcal and fermentative granulation of the organic colibacillus.

Thus, the fact observed by **Charrin** and **Veillon** becomes understandable. They observed pneumococcus in a peritoneal exudate on a living subject and found, at autopsy, only Bacterium coli, which was not there before.

F. Bezançon (6, p. 544) explains this fact by an agonal infection of intestinal origin. However, agonal infections do not exist. They are an imagined, but unproven, explanation to reconcile bacterial multiplication during agony with the false pasteurian dogma of asepsis of the animal organism.

In reality, the fact observed by **Charrin** and **Veillon** is explained by the transformation, during agony or after death, and under the influence of the drop in body temperature, of the pneumococcus, the normal granular form of the organic colibacilli, into its bacillary form.

This fact is completely identified with the observation of **Le Fèvre de Arric** who, isolating distinct colonies of streptococci and colibacillus from a wound on agar, obtained a distinct culture of each of them by transferring them separately to broth, and obtained only colibacillus alone when he inoculated them together in the same broth, a result due to the fact that streptococcus and colibacillus are two forms of the same microorganism.

The capsule of the pneumococcus is the blood fibrin that it has coagulated around itself. Proof of this is immediately provided by transporting its culture from an albuminous medium, where the capsule forms, to a medium deprived of the elements of fibrin where it immediately disappears.

If it has been found that the most favorable culture medium is serum, or a broth-blood or broth-serum mixture, it is because the normal habitat of the pneumococcus is blood, of which it is the fibrinferment.

Cause of Lobar Pneumonia

This explains, along with the nature and origin of the pneumococcus capsule, its constant presence in the sputum of pneumonia sufferers, where it is passively brought by the blood extravasated into the alveolus and not by dust in the air. Being a normal element of the blood, and the sputum at the beginning of pneumonia always containing blood, the pneumococcus cannot therefore not exist there.

If we consider that the presence of blood in the sputum is not immediate in pneumonia, that it is preceded by other symptoms, such as a stitch, the initial chill, and a rise in temperature, we must conclude that the organic lesion is already developing before the pneumococcus has penetrated the alveolus; that, consequently, it is not the primary causal agent of lobar pneumonia, and that its presence is only a consequence of the lesion.

Wurtz's experiments, relating to the influence of cold on microbial multiplication in the organism, the influence of cooling the body during the agonal period on the multiplication of the colibacillus in the blood and organs, the influence of cold on the vitality of tissues, and the necrosis of tissues exposed to prolonged cold, are of a nature to attribute a causal effect to cold.

There are indeed quite a few cases where cold has indeed appeared to be the cause of lobar pneumonia, either by acting through the air or directly on the chest wall.

Cold can act primarily either by causing a multiplication or transformation of the elements of the blood colibacillus, or by an alteration of the alveolar epithelial cells.

In this case, the cold, the primary cause of pneumonia, would cause a reaction of the colibacillary or the dumbbell organelles which would cause capillary hemorrhage and hepatization, the resorption of this and of the alveolar contents also being due to the autolytic action of the colibacillary granulations.

Trauma to the chest wall, especially when it causes rib fractures, can be the determining cause of pneumonia if there is lung damage. In this case, the rupture of the blood capillaries and the penetration of blood into the alveolus are due solely to the trauma and not to an inflammatory reaction caused by the pneumococcus; the latter exists in the alveolus only because it was passively brought there with the blood.

Pneumonia of traumatic origin constitutes precisely proof of the lack of specificity of the pneumococcus.

The constant presence of pneumococcus in pneumonia-related sputum may have led to its being considered the specific agent of pneumonia before it was known that it is the micrococcal granulation of fibrinferment and, at the same time, the micrococcal form of organic colibacillus. But now that the colibacillary function is known, as well as the nature of fibrinferment, it leads to the notion that, wherever blood or blood plasma penetrates, wherever there is a humor containing fibrinogen or globulins of a similar constitution, pneumococcus can be found.

This is why it exists in saliva and in the mouth, where it is not virulent, and why it is found in many diseases or lesions, pleurisy, pericarditis, meningitis, peritonitis, arthritis, etc. In all these cases, the pneumococcus has no specificity, and it is not the determining cause of the lesions, where it exists only because it is mechanically carried along with the collected fluids, in which it only takes the pneumococcal form because it has the coagulating power of fibrinogen and because the fluids contain fibrinogen.

The enterococcus does not differ from the pneumococcus, and I am convinced that anyone who tries it will easily succeed, by appropriate changes in conditions and cultures, in making a pneumococcus from a staphylococcus or a streptococcus, and, on the other hand, in making the colibacilli from each of them.

I am sure of this because these transformations are continually taking place in the body: the colibacillary granulation of the blood (fibrinferment) is carried into the saliva where it becomes pneumococcus; with the nasal mucus and ocular secretion where it becomes staphylococcus or streptococcus; all of this, carried into the intestine with all the digestive juices, is the origin of the intestinal colibacillus.

The intestinal colibacillus, continually carried out with the excrement, is constantly renewed by the colibacillary granulations brought by saliva, gastric, pancreatic, and intestinal juices.

It is exclusively pasteurian dogmas, and in particular that of the asepsis of living organisms, which have created an absolute impossibility of knowing the nature of the fibrinferment and the colibacillary function, as well as the multiple forms of colibacillus and their origins, and of perceiving that, if all these bacterial forms exist constantly in the mouth, the nasal cavities, and the pharynx, where they are not virulent, it is because they are brought there at all times by the internal environment.

* * *

Thus are fully justified the two conclusions drawn in the first volume of this work (p.475) concerning pneumonia, according to which the pneumococcus is not the agent, but only a witness to the disease and that it is not an agent of heterogeneous infection coming from the external environment.

My conclusion was completed by this inaccurate assertion that the pneumococcus is a bacterial element, born spontaneously in the human organism.

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The new concepts contained in this book prove that the pneumococcus, which is the colibacillary granulation of the blood, is not born spontaneously, but exists there normally and is a constituent element of the organism, of as capital importance as the dumbbell organelle, builder of tissues. It is therefore not born there, it always exists there.

The pneumococcus therefore does not come, as I had written following the microscopic examination of the composition of the sputum of pneumonia sufferers, from the segmentation of the mycelial filaments they contain. It is brought there directly by the blood, of which it is the fibrinferment; **it is therefore not an abnormal form of modified organic mold. It is a normal element of the organism.**

These corrections have the effect of precisely completing the demonstration of the nature and origin of the pneumococcus and of confirming the accuracy of my conclusions on its lack of specificity in pneumonia, and on its quality as an autogenous element.

* * *

ENTEROCOCCUS

The enterococcus is the organic colibacillus and is confused with it, as well as with the streptococcus and the pneumococcus, from which it differs in no way, either in morphological appearance or in properties.

Its normal habitat-the mouth, nose, pharynx, intestine, and peritoneal cavity-classifies it, like streptococcus, staphylococcus, and pneumococcus, among the forms of the normal colibacillus of the blood, from which it originates and indicates that it is poured into these cavities by the mucous or serous glands that line them and by the salivary glands.

Like the pneumococcus, it appears encapsulated when it vegetates in a liquid containing fibrinferment, and it loses this capsule in liquids that do not contain it; this capsule is a fibrin shell with which it surrounds itself because, like all micrococcal forms that are vegetative forms of the colibacillus, the enterococcus has retained the fundamental property of the colibacillary cocci of the blood, that of coagulating fibrinogen.

Enterococcus is known to coagulate milk; the fact that it surrounds itself with a fibrin shell in liquids containing fibrinogen is certain proof that it coagulates blood.

Enterococcus is essentially polymorphic; in its cultures, depending on whether they are young or old, we find isolated or grouped cocci, diplococci, tetrads, chains of 3 or 4 cocci, chains of 6, 8, or 10 cocci, chains of diplococci, chains of short bacilli, chains of very short diplobacilli, bacilli of various lengths, and germinative masses.

This variety arises from the fact that the germinative masses emit long mycelial filaments that segment into chains of bacilli, segmentation continuing in these until the state of diplobacillus (penultimate segmentation) or cocci (last bacillary segmentation); the segmentation, continued in the cocci, forms smaller grains.

These chains of segmented elements dislocate in very variable ways, thus releasing either entire bacilli or more or less long chains of diplobacilli, diplococcus, or cocci. The bacilli released individually, which segment in turn, form short chains of 4 cocci or 2 doplococcus, or give rise to 4 isolated cocci.

Some bacilli that do not segment show one or two light spots and a terminal granulation at each end and sometimes a third in the center. They are colibacilli, the entire culture being a colibacillary culture which, by the diversity of its elements, is at once a culture of staphylococcus, streptococcus, pneumococcus, tetragenus, and colibacillus.

Those who doubt the accuracy of this statement can convince themselves by repeating the observation of **Le Fèvre de Arric** who, simultaneously inoculating in the same broth colibacillus and streptococcus isolated from the same wound, found only colibacillus. Let them simultaneously inoculate in the broth two species of the colibacillary series,

even three, and they will realize the same fact; it may even happen that they find none of the inoculated species and that they find colibacillus in their place, thus renewing the observation of **Charrin** and **Veillon** who, observing pneumococcus in a peritoneal effusion during life, found only colibacillus there at autopsy, or that of **Mosny** and **Marcano** who, by injecting staphylococcus toxin into animals, caused the formation of abscesses and colibacilli peritonitis.

The explanation for these phenomena is as follows: by simultaneously inoculating two or three different forms in the same broth, for example tetragen, streptococcus, and pneumococcus, their elements participate in common in the formation of germinative masses and merge there, this because the three forms have a common specificity: the colibacilli.

The mycelial filaments, which emanate from these germinative masses, thus lose the exclusive character of each of them, and can, by segmentation, either reproduce exclusively one of the three forms, or none of them and give rise to typical colibacilli.

Let us say, in conclusion, that the fact of not having recognized the links which connect all these numerous forms of colibacilli is one of the misdeeds of the false dogma of monomorphism, a false doctrine which is the negation of the extreme polymorphism of living matter, an essential property which determined its upward evolution up to higher beings.

TETRAGEN

Its normal habitat, the oral and nasal cavities, indicates that, like the other forms of colibacillus, of which it is a member, it originates from the blood-borne colibacilli cocci and is shed into these cavities by the glandular juices.

It is an encapsulated diplococcus that has undergone longitudinal segmentation that has led it to the tetrad form.

Its fibrin shell attests that, like other colibacillus micrococcal forms, it coagulates blood; it also coagulates milk (**Sever, Sterling**).

Like other forms of colibacilli, it loses its shell in a culture medium containing neither fibrinogen nor globulin, and can lose its tetrad form there, to take on that of the staphylococcus and even others.

It also presents *variabilis, subflavus, aureus, citreus,* and *ruber* forms, which show that it is probably a staphylococcus that evolved in the nasal cavity and which, through this evolution, may have been modified in several of its properties; moreover, the tetragenic form is not at all characteristic; it is found in enterococcus and in broth cultures of milk cocci, evolving into *Bacillus lactis aerogenes*.

PNEUMOBACILLUS

The pneumobacillus is a form of the colibacillus; this quality undoubtedly results from the fact that its identity with Escherich's *Bacillus lactis aerogenes* was demonstrated by **Denys** and **Martin**, **Grimbert** and **Legros**, **Bertarelli**. Now, the *Bacillus lactis aerogenes* is the bacillary evolutionary form of the milk colibacillus cocci, the agent of lactic acid production and spontaneous coagulation of milk. The milk colibacillus cocci is the blood colibacillus cocci (fibrinferment) that has passed through the mammary gland.

Note further that since the *Bacillus lactis aerogenes* has been recognized as a species belonging to the colibacillus group, it follows, as a result of this identification, that it a priori includes the pneumobacillus.

The presence of a capsule attests that the pneumobacillus coagulates blood; it coagulates milk.

It has, on the other hand, the same action as the colibacillus on sugars, notably glucose, sucrose, lactose, maltose. A clear distinction is currently being established between the pneumobacillus and the colibacillus, based:

1. On the fact that the colibacillus alone is mobile in broth cultures; this character has no value; no bacillus is actively mobile; mobility is only passive and comes only from the fact that a granulation with Brownian movement has attached itself to the bacillus and is dragging it along in this movement

2. On the absence of a capsule in the colibacillus. The colibacillus does not have a capsule due to its quality (fibrinferment) and the coagulating function of its cocci in the blood, a function that can only be performed on the condition that fibrin is not deposited on them, because it would clump them into large masses and the blood would coagulate in the vessels. There is therefore an adaptation of the function of the cocci to the environment.

The blood cocci (fibrinferment) retain their coagulating power when they are carried out of the vessels into glandular juices, for example, saliva or milk, but in these liquids, they have changed their environment and can then exert their power. It is because of this that they surround themselves with a shell if the environment contains fibrinogen or globulins, or that they coagulate en masse the liquid in which they live (milk).

The presence or absence of a shell is therefore not a characteristic that can be used to distinguish the pneumobacillus, nor the pneumococcus from the colibacillus, all endowed with the same coagulating power for fibrinogen. The milk cocci evolve into *Bacillus lactis aerogenes*, proven to be identical to the E. colibacillus, the cocci of the latter also

evolving, either into bacillary form or into streptococcal chains, in the serum left to itself, just as the milk cocci, inoculated in broth, evolves, exactly like the enterococcus, into isolated cocci, into diplococci, into chains of 8 to 12 cocci or diplococci or short diplobacilli, and also into longer or isolated bacillary elements which are the *Bacillus lactis aerogenes* or colibacillus.

All these facts, which appear so complicated, which are contrary to the current notions of bacteriology, are however of great simplicity and all arise from the same following cause: the false dogma of bacterial monomorphism which has led bacteriologists to erect into different bacterial species, bacteria which only differ from each other by minimal, unstable characteristics, and which all come from the same strain, which is the organic colibacillus of the blood (fibrinferment); this is the general pro-ferment of all digestive ferments, and also the source of all general bacterial elements which the normal or abnormal liquids of the organism contain, this apart from the state of diseases due to a heterogeneous virus.

Bacteriologists have been prevented, by their faith in the false pasteurian dogma of the asepsis of living organisms, from even being able to suspect such facts and from knowing that the secretion of glandular juices is accompanied by the passage, into these liquids, of cocciform bacteria contained in the blood and which are normal constituent parts of the organism, however reputed to be aseptic.

In summary, about twenty of these supposedly distinct and characterized bacterial species are the colibacillus, which appears in various forms, varies with the culture media and according to the points of the organism where it is sampled.

3. On the fact that the pneumobacillus does not produce indole. The production of indole is a very irregular property for the various forms of the colibacillus; it is a property that seems to develop especially in an anaerobic environment; the staphylococcus produces it, but the enterococcus and the pneumobacillus produce little or none. The intestinal colibacillus of humans produces a significant amount, that of the blood much less, the colibacillus of herbivores generally produces little, but, nevertheless, horse urine contains a quantity twenty times greater than that of humans.

In summary, the biochemical properties of the colibacillus are as variable as its morphological appearance; by changing environment, its properties vary and adapt to the new conditions; the production of indole is one of the properties that is most inconsistent in the micrococcal forms of the colibacillus.

* * *

The ozena bacillus is so similar to the pneumobacillus that the identification of the two microbes has already been made by several bacteriologists; with the rhinoscleroma bacillus, it is identified with the colibacillus.

SEPTIC VIBRIO

I do not need to describe here its properties or characteristics, which are known. I will only observe:

1. That it completely resembles the colibacillus in cultures and, like it, has central spores or spores placed at the end of the bacillus.

2. That, like the colibacillus, it loses its vitality in a few minutes at 58-60°C.

3. That, like the intestinal colibacillus, it produces a lot of indol.

4. That it is not at all a strict anaerobe and that it cultivates well in environments that are incompletely deprived of air; moreover, **Rosenthal** was able to adapt it to aerobic life.

5. That the aerobic septic bacillus of **Legros**, observed by this author in two cases of gas gangrene, is exactly **Pasteur's** septic vibrio and that it presents all the characteristics of the colibacillus.

6. That it normally exists in the intestine of humans or animals.

7. F. Bezançon states in his "*Précis de Bactériologie*," as pasteurian dogmas dictate, that: *After death, it passes from the intestine into the blood.* This is an error. Putrefaction takes place after death because all parts of the body normally contain the septic vibrio, *which is the organic colibacillus*, and not because the bacterial elements of the intestine cross the intestinal mucosa to pass into the blood of the corpse.

The long filaments of the septic vibrio normally exist in the blood, although rare, as do shorter bacillary elements; but they do not develop hydrogen sulfide or putrefaction there because they live in a highly oxygenated environment. In the corpse, they become septic vibrios, and putrefaction occurs there because the environment becomes anaerobic.

It is not the septic vibrio or the tetanus bacillus that exist in the soil; it is the various elements of the colibacilli of humans and animals, and it is these that, transferred to an anaerobic culture medium, give rise to the septic vibrio and to the tetanus bacillus;

8. The cilia of the septic vibrio are those of the colibacillus. They are not cilia; they are fine mycelial filaments that originate from rounded, oval, or more elongated germinative masses, formed by the agglomeration of granules and bacillary elements of the colibacillus.

9. The aerobic septic bacillus of **Legros** coagulates milk like the colibacillus and like its staphylococcus form. The septic vibrio will certainly also coagulate it, as well as blood,

by bringing it to the micrococcal form by aerobic culture. It is the presence of this form in a culture that is necessary to achieve the coagulation of blood and milk.

The septic vibrio is, in summary, the organic colibacillus which, in an anaerobic environment, mainly takes on the bacillary and filamentous forms which are also observed in the blood of normal individuals; to see them, they must be looked for in the white layer which covers the blood cells after centrifugation of the blood, or, as **Béchamp** did, in blood collected aseptically in an oxygen-free atmosphere and placed in an incubator for several days.

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It is the organic colibacillus that produces the putrefaction of corpses; this begins in all parts of the body simultaneously since the colibacillus exists everywhere; it develops more quickly in the most aqueous or least compact environments.

When a tourniquet is applied to a limb whose artery is severed, the circulation of blood is completely stopped. The blood contained in the capillaries and veins loses all of its oxygen **fairly quickly**; the same is true for the plasma circulating in the tissues and cells, whose nutrition is, from that moment on, completely stopped.

Then begins the anaerobic development of the organic colibacillus, which, by this fact, becomes the septic vibrio; the fermentative properties of the colibacillus are not affected; but, instead of developing in an oxygenated environment, where it produces only the well-known normal gases and bodies that are assimilated or rejected outside, the

fermentative reactions that produced in an oxygen-deprived environment, develop hydrogen, hydrogen sulfide, nitrogen, indole, etc., which produce gaseous edema of the connective tissue.

It is stated elsewhere that, through a misinterpreted experiment, **Pasteur** believed he had demonstrated that blood is aseptic, and that when left in a sterile vessel in contact with air, it does not putrify. **Béchamp** demonstrated that, on the contrary, blood is, under these conditions, the site of fermentation phenomena. And, in another experiment, the results of which will help us understand the transformation of the colibacillus into a septic vibrio, he demonstrated:

1. That in the presence of oxygen, bacillary-form bacteria do not develop in blood stored at room temperature or in an oven;

2. That when blood is mixed with a little water saturated with carbonic acid and kept in an atmosphere of this gas, numerous bacilli develop in it in two or three days, and, he says, long mobile filaments similar to leptothrix. These are the long colibacillary filaments that normal blood already contains, where they are rare, and they are also those

of the septic vibrio; plates 111, figure 5, first volume, and 74, figure 9, third volume, show one of them as well as bacillary elements (plate 74, fig. 8, 3rd vol.), in the normal blood of the dog.

Let us recall that **Béchamp** did not admit that the molecular granules that he had named microzymas were of a bacterial nature, but that he affirmed that they nevertheless have the power to transform themselves into bacillary elements.

In any case, **Béchamp's** experiment demonstrates that, in the presence of oxygen, vibrios (bacilli) do not develop in the blood and that they do develop as soon as the environment becomes anaerobic.

This fact implies that, in the presence of oxygen, that is to say in normal blood, the colibacillary cocci, the nature of which **Béchamp** was unaware, remains in this state which is the one necessary for it to play its fermentative role, and that, as soon as it is in an anaerobic environment, it develops in the bacillary and filamentous form; the septic vibrio therefore appears in the blood of a member deprived of circulation or in a corpse, as soon as the oxygen has disappeared.

In summary, the colibacillus normally exists in the blood in the bacillary state and in the state of long mycelial filaments identical to those of the septic vibrio with which it identifies completely and specifically, just as it identifies with the *Bacillus perfringens* of **Veillon** and **Zuber**, identical to the bacillus described by **Achalme** in 1891 in acute articular rheumatism, and with the aerobic septic bacillus of **Legros**; but these bacillary and filamentous forms are not very numerous in normal blood. It is they which develop greatly in gas gangrene.

The species described under the names *Bacillus ramosus, Bacillus serpens, Bacillus tethoïdes, Bacillus fragillus, Bacillus fusiformis, Staphylococcus parvulus, Micrococcus foetidis,* are certainly all vegetative forms of the colibacillus, which have no distinct and fixed specific character, and which can, by changing their environment, completely transform themselves and take on other forms of the colibacillus.

The most important, *Bacillus fusiformis* or *Vincent's bacillus*, found in hospital rot, is incapable of reproducing this rotten rot by inoculation into humans, simply because it is a vegetative form of the normal organic colibacillus of humans and is therefore not pathogenic to them. If we find this bacillary form in hospital rot, it is not because it is this which causes this rot which, in reality, like gas gangrene and all gangrene in general, always results from another cause which is that which has deprived a given region of the body of circulation and nutrition; once the circulation of the blood is stopped, the colibacillary elements present in the affected region continue to exercise their fermentative action there, accumulate the products of this action and thus determine the rot of the region exactly as, in the corpse, putrefaction begins quickly after the cessation of

circulation by the evolution of the normal colibacillus, this beginning even during the agonal period.

If we have not understood until now these so-called agonal infections, nor cadaveric putrefaction, nor gas gangrene, nor hospital rot, nor the presence nor the nature of the bacteria observed in gangrene and suppurations, nor the nature nor the cause of the development of the septic vibrio, tick, it is exclusively the false dogma of the asepsis of living beings, established by **Pasteur**, which is responsible.

This is how, by establishing this false and harmful dogma, by violently combating the fermentative properties of **Béchamp's** microzymas and their evolution into vibrios, **Pasteur** made it impossible for himself to understand the cause of gas gangrene, the nature and origin of the septic vibrio, and he and his school made it impossible to detect the existence of the organic colibacillus and the capital colibacillary function, to know the real and original form, as well as the original source of viruses... etc. By developing this dogma, he has destroyed, to this day, the possibility of progress in bacteriology and the fight against diseases.

* * *

In summary, all gangrene occurs not because the septic vibrio develops in the affected region, but only because the tissues of the region are deprived of circulation, nutrition, and oxygen; this region, regardless of its location in the body, always contains the elements of the septic vibrio, which are those of the normal colibacillus; they develop in the filamentous and bacillary form exclusively because the environment quickly becomes anaerobic.

The septic vibrio therefore has no specific existence; it is the bacillary form of the colibacillus, whose vegetation becomes luxuriant and filamentous in anaerobic environments and develops abnormal fermentations there.

* * *

This identification of the colibacillus, which has just been explained, results from the new knowledge of the existence of the organic colibacillus as a normal constituent of the animal organism and of the existence of the colibacillus function.

In the first volume of this work, I presented a different and inexact identification for the septic vibrio and for tetanus, based on the study and comparison of various conidial forms. My conclusion was led by obtaining, by inoculation on solid media and in contact with air, pure cultures of septic vibrio and tetanus. I thus obtained, in both cases, a hyphomycete of a very special, characteristic conidial form, not described in treatises on Mycology, which I called *Corymbisporium b*ecause of the shape of the branched spore-bearing filaments. This form is photographed in figures 1 and 2 of plate 208 (conidial

form of the septic vibrio) and 1 and 2 of plate 207, first volume (conidial form of the tetanus bacillus).

I had also obtained this *Corymbisporium* form from the carrot, and from this vegetable only (pl. 260, fig. 17, 18, 1st vol.), but I later recognized that it differs clearly, in certain characteristics, from the form obtained by evolution of the septic vibrio and the tetanus bacillus. My identification was therefore false; the error also results from an insufficient study of the bacterial forms and their biochemical reactions. But it should not be forgotten that the vast study of virus identification that I had undertaken had the main aim of establishing and gathering the materials necessary to carry it out.

The same rectification was made for the tetanus tetanus bacillus which, like the septic vibrio, is the anaerobic culture of the colibacillus which can easily be reduced, by culture in the open air, to the ordinary bacillary form of the colibacillus.

* * *

Finally, I recall that the identity of nature between **Pasteur's** gas gangrene or acute experimental septicemia, Koch's malignant edema, and symptomatic anthrax in domestic animals is currently accepted. The bacterial agent of symptomatic anthrax tumors is certainly, as in gangrene or malignant edema, the organic colibacillus of the sick animal.

The properties and forms of *Bacillus Chauvéi* in fact coincide with those of the colibacillus. But, in this case, the initial cause of the tumor remains unknown; it is not known whether it results from an obliteration of the vessels leading to a stoppage of circulation and nutrition in the diseased region, or whether it is an alteration or degeneration of the fundamental properties of the organic colibacillus or the dumbbell organelle.

CADAVERIC AND AGONIC INFECTIONS

Here are the known facts according to **F. Bezancon** (*Précis de Microbiologie clinique*, p. 543): **Wurtz** and **Hermann** found colibacilli in the liver, spleen, and kidney of humans 24 to 36 hours after death, 16 times out of 32 autopsies.

Lesage and **Macaigne**, **Welsch** of Baltimore, **Marfan**, with **Manu** and **Marot**, observed, on the other hand, that colibacilli is only found in the viscera during autopsies when there are intestinal lesions and claim that, if **Wurtz** and **Hermann** found it so often, it is because they operated in the summer.

Here, as always, the results and discussions have been distorted by convictions resulting from false dogmas that impose the notion that, during life, the animal organism is aseptic.

It is obviously the assertion of **Wurtz** and **Hermann** that is in accordance with reality, because formal proof of it is given by the fact that, the organism of mammals being obviously of a nature no different from that of man:

1. A portion of their liver, spleen, kidney, or other organ, taken aseptically from a living and normal animal, then reduced to a pulp and inoculated on various media, gives rise to a culture of the coli type in one of its forms, bacillary, colibacillary, or micrococcal;

2. The inoculation of the granules and platelets that can be removed from normal blood or serum gives rise to a culture of the coli type;

3. A portion of normal coagulated milk gives rise to a culture of the coli type;

4. In oxalated plasma, or in fibrinogen solutions, a culture of the coli type spontaneously develops, which leads to spontaneous coagulation;

5. Microscopic examination of the white layer that covers the blood cells in centrifuged blood shows an enormous number of granulations identical to micrococci, cocobacilli, bacilli, and mycelial filaments not yet segmented, all of which are the various elements of the organic colibacillus, a normal and fundamental element of animal organisms;

6. Microscopic examination of a piece of clot, crushed between two slides, shows an enormous number of granulations, whose appearance does not differ in any way from bacterial micrococci and which, in reality, are nothing other than the elements that, under certain conditions, become staphylococci and streptococci.

Thus, we see in the blood where they normally abound the elements of *Bacterium coli* and we cultivate them easily. How, under these conditions, and in the face of the multiple proofs that I have just listed, could one allege that if, after death, they are found in the corpse, it is because they have crossed the intestinal mucosa and invaded the organism from near to far? This allegation is not only false, but is not even plausible, because if coli germs existed only in the intestine of the living being, it would take at least 8 days, perhaps more, for them to spread to the spleen, the spinal cord, the extremities of the limbs, etc.

Moreover, **Letienne** found *Bacterium coli* in the bile of 3 cadavers out of 42 examined, 45 minutes after death.

Reco found bacteria in the spleen 20 out of 27 times, 15 to 45 minutes after death.

Experimenting on animals experimentally poisoned by arsenic acid, **Wurtz** saw that, 5 times out of 7, microbes appear in the heart blood and peritoneal serosity when their temperature drops between 32°C and 290°C. He thus observed *Proteus vulgaris* 31 times, coli 9 times, then sarcina, a streptococcus, and an unclassified microbe.

Beco observed the same facts in poisonings by cantharide, cantharidin, and emetics, and the invasion always occurs, according to him, during the agonal period.

He also observed that *Bacterium coli* multiplies after death and **predominates over other species to the point of masking them,** a fact which explains why, some time after death, one can find a different result from that of an examination carried out during the agonal period.

In reality, there is no different result: there is only one and the same element whose form has changed. The normal form of organic coli in normal individuals and at their temperature of 37 to 38°C is the cocci form of various sizes. When the temperature changes and drops, they change form and pass into the bacillary state, while the cocci forms become rarer. The fact is easy to observe. There is therefore no predominance, but only a change of form.

Beco, on the other hand, isolated *Bacterium coli* in the thyroid body during the agonal period 9 times out of 11.

Achard and **Fulpin** removed colibacilli six times and Staphylococcus twice from the liver during the dying period, microbes that were not the cause of death. However, since Staphylococcus is a form of colibacilli, it was always the latter that they observed in the liver.

A very interesting study by **Dallemagne**, reported by **Bezancon**, shows that in the last days of life, under the influence of a restricted diet, the microbes brought by food decrease, then disappear, and that, finally, only the permanent hosts, colibacillus and staphylococcus blancis, remain in the intestine. The latter is coli bacilli in the cocci state.

F. Bezancon observes that it is precisely these two species that are the usual agents of agonal and cadaveric infections, whose intestinal origin is universally accepted.

This false conclusion has been accepted because it emanates from **Pasteur** and because his false dogma of the asepsis of living beings imposes it. The six conclusions stated above and the entire contents of this work demonstrate peremptorily, beyond dispute, the falsity of this dogma; not a single exact fact can be invoked to establish its accuracy; until now it has been supported only by worthless arguments and by unjustifiable convictions.

The conclusion to be drawn from all these facts is that their explanation has been distorted by the incriminated dogmas and that the mechanism of their production is explained in a clear, luminous way by the incontestable existence of the organic, rnanic colibacillus, a constituent element of the animal organism of capital importance.

This new fact naturally explains a large number of others, noted by bacteriologists and biologists, and which, in order to remain in accordance with the dogmas, had to receive false interpretations. The conclusion of this study is therefore that the facts presented demonstrate the constant and normal presence of the colibacillus in all parts of the animal organism during life and that there is no agonal or cadaveric infection, but only the evolution and multiplication of the colibacillus which, itself, remains alive after the death of the being's organization; its multiplication begins as soon as the temperature of the organized body falls below that which is necessary for the maintenance of its physiological form (coccus form) and its normal functions.

This last notion is of considerable importance because it explains the development of autogenous colibacillary diseases a frigore (coryza), respiratory tract diseases, pneumonia, etc.

Let us also note, to conclude, that the existence of organic colibacillus, in its various forms in normal blood (bacillary and micrococcal), now provides an explanation for the enormous progress that the improvement of hemostasis has brought to the results of surgical interventions, progress which is only partly due to aseptic precautions.

It is the blood and lymph spilled into wounds and serous cavities which, through the colibacillary germs they contain, develop suppuration. The better hemostasis is achieved, the greater the speed of healing and repair.

* * *

CONCLUSIONS

1. The development of putrefaction in corpses through invasion of the body by microorganisms from the intestine and the surface of the skin is an erroneous assertion by **Pasteur** for which he did not provide the slightest element of proof;

2. The putrefaction of corpses develops because the organic colibacillus, a constituent element of capital importance which exists in all parts of the organism, multiplies actively there as soon as circulation stops in the bacillary form and in the form of the septic vibrio;

3. The multiplication of the organic colibacillus in the bacillary form begins already during the agonal period when the cooling of the body reaches a value close to 32°.

CHAPTER X

STUDY OF THE NATURE, ORIGIN, AND PROPERTIES OF FIGURED FERMENTS OR DIASTASE

I. Morphology of diastases.

II. Origin of diastases.

III. Multiplication and morphological transformations of diastases during their activity.

IV. Properties of diastases. Multiple activities of the same fermentative element.

V. The colibacillary function of mammals, general bacterial function of living beings.

VI. The micrococcal granulation of the organic blood colibacillus is the active granular element of the diastases of digestive juices

VII. The micrococcal colibacillary elements, transferred by the blood to the digestive and glandular juices, are the origin and source of staphylococci, streptococci, pneumococci, etc., which normally inhabit the nasal, oral, and pharyngeal cavities and the digestive tract, as well as of intestinal colibacillus, the latter by evolution from cocci.

VIII. These are the micrococcal colibacillary elements of the blood (fibrinferment) which, after injection of the blood serum of one animal into the blood of another species, develop in the latter the proteolytic ferment discovered by **Abderhalden** and which create the state of anaphylactic sensitization as well as the precipitating power of the serum of the recipient animal towards the antigen serum.

IX. The fermentative part of the complement or alexin is the colibacillary granulation of the blood, that is, the fibrinferment.

* * *

I. Morphology of diastases

Diastases are figured, granular elements of a bacterial nature, with a diameter varying from approximately 0.2 to 0.8 microns, basophilic when they are rich in chromatic matter, acidophilic or neutrophilic when they contain no more or only very little.

There are no non-figured ferments, so-called soluble and non-figured ferments. They cannot exist for the following reasons:

1. A substance that is entirely soluble in certain liquids, precipitable by others, and that is not retained by any filter, however fine, cannot exert a fermentative action. It can only participate in purely chemical actions with other substances, actions strictly limited by the invariable mass of substance involved, and terminated by its exhaustion.

2. The characteristic of a diastase is **to possess, under a determined mass, the power to transform an unlimited quantity of substance by a fermentative action.**

3. This unlimited power results from the fact that the diastase is alive and actively multiplies. It can only multiply if it is made up of figured elements. **Diastatic power can therefore only belong to a figured and living element.**

4. There is an essential difference between a diastase and a chemical catalyst; the latter cannot multiply and requires its recovery after each operation to be able to theoretically transform an indefinite quantity of substance.

Any granulation of living matter, spherical, with a size varying from 0.1 to 0.8 microns is of a diastatic and bacterial nature. Because it is alive, it is capable of multiplying.

II. Origin of diastases

All living organisms are made up of two living organelles, each fulfilling a well-defined function:

A) The dumbbell organelle, an already differentiated, structured element, formed by a rod bearing a ball at each end and which, through its articulation exclusively with the balls of other dumbbells, forms filaments and polyhedral networks that constitute the tissues of living beings.

B) The micrococcal organelle, spherical in shape, 0.2 to 1 micron in size, possessing the fermentative property and fulfilling the function of carrying out the chemical actions necessary for the maintenance of life.

These two organelles are capable of multiplying. They are therefore alive.

Some biologists, such as **Cuenot** for example, have claimed that the cell is the first form, the first level of living matter, that life exists only in organization, the organelles that constitute the cell not being alive; **Nageotte**, in *The Organization of Matter in its Relations with Life*, approves and attempts to justify this theory, which is contrary to the facts.

In the first volume of this work, I demonstrated the fact that a bacterial culture filtered through porcelain, which has been improperly called a toxin, containing only the micro-

coccal elements of the culture, is easily returned to the bacillary state, then to the hyphomycete state.

A mycelial filament of *Mucor*, of *Aspergillus*, is organized, constituted by dumbbell organelles assembled by their balls and gives rise to fermentative micrococcal elements. The mycelial filaments and spores of a hyphomycete of the genus *Botrytis* give rise in broth culture at 370, to a bacterial culture containing bacilli and micrococci and, on agar, at 20-25º, to sclerotia which, placed at 1 centimeter depth in moist sand, germinate there giving rise to an organized and ascosporous fungus, *Sclerotinia Fruckeliana*.

Thus, the same living matter easily passes from the micrococcal state to the bacillary state, then to the hyphomycete state, then to the organized fungus state, and it would not be alive in its bacterial forms? These facts peremptorily demonstrate the inaccuracy of **Cuénot's** opinion or hypothesis.

Organic matter is alive as soon as it possesses both a well-defined figurative form and the property of multiplying. Two other characteristics of this living matter are: the fermentative property and the fact of losing it at a temperature generally varying from 45 to 60°C in a liquid medium, a loss probably due to the fusion of the ethers of cholesterol at these temperatures and to a corresponding change in the chemical organization of the molecule in the micrococcal elements.

Due to the elementary constitution of living beings, it follows that the diastases that we observe and that we can extract from any living being, animal or plant, are the micrococcal element of its normal bacterial function, which is the colibacillary function in mammals and vertebrates.

Diastases do not result from a secretion of the tissues of living beings as is believed; they are an integral part of them as a figurative element and a normal constituent of their organism.

More generally and, in principle, ferments are the micrococcal form of bacteria.

* * *

III. Multiplication and Morphological Transformations of Diastases During Their Activity

In a nutrient medium, the multiplication of the micrococcal elements of the ferment takes place, initially, by germination of each granulation, which emits a short, divaricate filament, bearing 3 to 6 or 7 fine, short-pedunculated spores, which I called zymospores in the first volume of this work and which subsequently detach.

The diastase then undergoes other transformations which were described in the first volume of this work (p. 185) in connection with the study of the evolution of barley diastase (pl. 144, 1st vol.) and pork pepsin (pl. 115, 1st vol.) cultivated in glucose peptone broth.

It is indicated in this study that the ferment granules clump together into germinative masses which emit long, highly branched mycelial filaments (fig. 5, pl. 114, 1st vol.) on which shortly pedunculated spores are born. The finest mycelial branches segment into bacillary elements of different sizes, straight or arched, which segment in turn, until they reach the stage of cocci.

The culture can also form a veil that passes into the hyphomycete state. Barley diastase easily passes into the *Penicilium* state, and pig pepsin into the *Penicilium* and *Aspergillus* states. In this volume, in the chapter on blood clotting, you will find an account of the multiplication of fibrinferment (serozyme) and its transformation into mycelium and bacillary elements, which are the colibacillus.

It is indicated further on that the micrococcal diastatic elements of the blood pass into the digestive juices, of which they constitute the active elements. The considerable volumes of digestive juices emitted in 24 hours in a human being (several liters) prove that the multiplication of these micrococcal elements must be extremely active in the body.

* * *

IV. Properties of Diastases.
Multiple Activities of the Same Fermentative Element

The same diastatic element possesses multiple fermentative properties. Until now, each chemical action of a fermentative nature has been considered to be due to a special diastase, which would be specifically linked to this reaction; but no proof has ever been provided, and no such diastase, endowed with only such a property, has ever been isolated from a liquid capable of also carrying out multiple and different actions.

Bourquelot and **Gley** found that *Penicilium glaucum*, cultivated on **Raulin** fluid, secretes a chymosin, a casease, a lipase, an amylase, an inulase, a trehalase, a maltase, and an invertin; but they did not provide proof of the specificity or exclusivity of action of each of these diastases; they provided no morphological indication that would allow them to be distinguished and to know whether it is a secretion of a non-figured soluble product or the development of granulations or cocci; they only observed, in **Raulin** fluid, the acquisition of fermentative properties, the quality of which they successively determined on different substances. In particular, they did not isolate any body possessing exclusively one of these diastatic actions.

It would be truly surprising if the same living matter, that of *Penicilium glaucum*, had at least eight varieties of cocci, each with a different diastatic activity. No evidence so far supports such a conclusion.

Gastric juice contains pepsin, a chymosin (lab ferment), and a lipase, that is, it possesses these three diastatic actions, which does not prove that it possesses three distinct diastases. According to **Pawlow**, it would be the same diastase that exerts the pepsin and labial actions.

Others claim there are two. There is certainly only one, and we will prove it:

Various authors have demonstrated that leukocytes contain multiple diastases: protease, amylase, lipase, oxidase, chymosin, etc. Now, it is demonstrated in this book that leukocytes are agglomerations of granules of the organic colibacillus, just like platelets; they therefore possess the same fermentative properties as these free granules. Furthermore, this agglomeration, which represents one of the stages of bacterial multiplication in general and, here, of the organic colibacillus in particular, obviously only occurs with granules of the same nature, of the same specific origin, and all intended to reproduce the same element. We must therefore conclude that, if an agglomeration of 3 or 4 dozen identical granules, of the same nature and origin, possesses 10 different diastatic actions, this means that each of them also possesses them on its own account.

Moreover, the blood that contains these same granulations, which are the active agents of the colibacillary function, possesses all these diastatic properties. They constitute the fibrinferment which, by hydrolyzing the lecithin in the blood, releases its acid group, the fixation of which on fibrinogen determines its precipitation in the form of fibrin;

This is therefore proof that the colibacillary granulation has the properties of a lipase. We also know that the fibrin thus formed, placed in a solution of sodium

chloride, undergoes an autolysis that **Dastre** called saline digestion and which, in reality, results from the activity of the fibrinferment carried with it; it therefore possesses proteolytic power at the same time as lipase power.

If the serum containing these colibacillary granules with proteolytic power is injected into the veins of another animal, it develops the same power discovered by **Abderhalden**, which is the active agent in anaphylactic sensitization.

These granules also constitute the glycolitic ferment of the blood, which is confused with the fibrinferment. They also possess chymosic power and coagulate milk, which indirectly coagulates the blood by acting on the lecithins it contains. **Dastre** observed that the lymph of the thoracic duct, which also contains leukocytes and granules, coagulates milk. They therefore possess proteolytic, lipase, chymosic, and glycolytic powers, among others.

Knowledge of the colibacillary function allows us to go further in our analysis. It is demonstrated, elsewhere in this volume, that staphylococci, streptococci, pneumococci, and enterococci are forms of colibacillary granulation; each of them possesses the power to coagulate blood and milk, and lipolytic, lipase, and proteolytic powers. For streptococcus, in particular, the chains of cocci, which are the result of the segmentation of a long mycelial filament born from a germinative mass, prove that these granulations, arising from the same filament and from identical filaments and germinative masses, all have exactly the same origin, the same structure, the same chemical constitution, and the same properties, which are multiple.

We will see later that this same colibacillary granulation that the blood contains and which, consequently, is widespread in the tissues and all the organs, is the active agent that determines the autolysis, inaccurately called aseptic, of the tissues in vitro or in the organism, even in the pathological state.

We can therefore conclude, from what has just been explained, that the same micrococcal or fermentative granulation possesses multiple diastatic properties that allow it to exert the most diverse chemical actions.

V. The colibacillary function of mammals, general bacterial function of living beings

It is indicated, in the chapter relating to the study of blood coagulation and the colibacillary function, how I came to know it by observing the granular and micrococcal nature of the fibrinferment, its morphological modifications in fibrinogen solutions and oxalated plasma, notably in filaments segmented into bacillary elements which are those of the colibacillus.

I therefore refer to Chapter VIII for all related questions. It will be seen that the colibacillus exists in the blood in the form of micrococcal granules, bacillary elements of various lengths, mycelial filaments, and germinative masses of various sizes; these are either clusters of small granules of the micrococcal form (platelets or hematoblasts), or spherical agglomerations of granules that have remained distinct and are 3 to 12 microns or more in size (leukocytes), or uniform spherical or irregular masses, basophilic, of variable size (small and large mononuclear cells), the latter emitting large mycelial filaments that segment into bacillary elements.

Leukocytes and platelets or hematoblasts are therefore the agglomerative or germinative masses of the organic colibacillus in the process of forming or already fully formed and ready for germination (large mononuclear cells).

The colibacillary micrococcal elements are the innumerable granulations that form blood clots by their union and agglutination, by means of fibrin filaments, in a network in the meshes of which the serum is housed.

Due to the presence in the blood of this innumerable quantity of colibacillus cocci, possessing diastatic properties, they are widespread everywhere, in all the tissues and organs of animals, of which they are not a secretion since they are normal constituent elements, just like the organelles constituting the tissues and cells.

This presence, in all parts of the body, of colibacillary cocci with multiple diastatic properties, explains the phenomenon of aseptic autolysis of tissues or **Salkowski** autodigestion, which consists of the complete liquefaction of a fragment of tissue, extracted aseptically and placed in an oven at 37°C in chloroformed water for a sufficiently long time. The autodigestion of proteins produces peptones and amino acids, that of nucleoproteins dissociates them into phosphoric acid and purine bases, that of lecithins into choline, glycerophosphoric acid, and fatty acids; fats are also digested, as are carbohydrates, which are transformed into lactic acid and alcohol.

Only the connective tissue and elastic fibers are not attacked; this is probably because, as I showed during the study of the vegetation of the fibrinferment, it is the micrococcal elements or spores of the organic colibacillus that give rise to the elastic fibers and bundles of the connective tissue.

This origin of these proves a common constitution with the colibacillary cocci, which explains why they are not attacked by them.

In aseptic autolysis, it is therefore the proteolytic, lipase, and glycolytic properties of the colibacillary cocci of the blood that operate the autodigestion; in the autodigestion of fibrin in saline solution, it is only the proteolytic property of these cocci that acts.

As all phenomena naturally follow one another with marvelous clarity and simplicity in the exercise of the colibacillary function, we will now see that it is these colibacillary cocci of the blood, possessing these diastatic properties which, crossing the digestive glands with a part of the blood plasma, come to constitute the active elements of the digestive juices and form ptyalin, pepsin and trypsin, etc.

VI. The microcrystalline granulation of organic colibacillus of the blood is the active granular element of the diastases of the digestive juices

During experiments carried out in 1903, with the collaboration of **G. Moussu** at the Alford Veterinary School, to determine the value of the expenditure caused by the activity of the parotid gland of the ox, I observed, from the first experiment, a curious phenomenon:

the arterial blood and the venous blood leaving the active gland, collected by mixing them separately, in special syringes with a determined volume of sodium oxalate solution, were, at the time of collection, the first, bright red, the second, much less so.

The extraction and analysis of blood gases, taking place in the Museum laboratory, could only be done two to four hours later; the gas extraction took place with **Chauveau's** double pump, which I described in the treatise on *Biological Physics* by **D'Arsonval** and **Chauveau**, and which allows the simultaneous extraction of gases from arterial and venous blood.

When I introduced the arterial and venous blood, taken in an active state, into the pump, I noticed that the venous blood was a little redder than the arterial blood. I thought I had made a mistake in labeling the samples. Blood gas analysis showed that the arterial blood contained only 4 or 5 cc of oxygen per 100 cc of blood, while the venous blood still contained 5 or 6.

The labeling of the samples was therefore especially careful in the following experiment, but the result was the same. The venous blood remained clearly redder than the arterial blood, and gas analysis showed that the former contained significantly more oxygen than the latter.

In a third experiment, blood samples were collected, some with a sodium fluoride solution added, others with a sodium oxalate solution; these were kept in syringes for 24 hours at laboratory temperature, before the gases were extracted. At the end of this time, the arterial blood had become completely black, and contained less than 0.5 cc.% of oxygen, while the venous blood, which had retained a very distinct red tint, still contained 4 to 5 cc.%. The arterial blood, with sodium fluoride added, treated two hours after collection contained about 12% oxygen.

These facts meant that the cow's arterial blood loses its oxygen more quickly than the venous blood taken at the time of parotid gland activity, when left to their own devices in vitro, protected from air. The venous blood leaving the active gland is therefore the site of less active combustion than the arterial blood, although, from a certain point on, it contains more oxygen than the latter.

The activity of the gland was determined by electrical excitation of the parotid nerve. The amount of venous blood and saliva flowing per minute from the gland was measured, either at rest or in an active state. The sum of the amounts of saliva and venous blood represented the amount of arterial blood entering the gland, albeit somewhat inferior, since it is to be presumed that a certain amount of blood plasma leaves the gland through the lymphatic channels. Depending on the intensity of the excitation and the state of fatigue of the gland, the amount of saliva secreted reached up to nearly 100 cc per minute (93 cc in one experiment), while the amount of venous blood leaving the gland varied from 120 to 130 cc.

Other experiments were conducted on dogs, by comparing arterial blood and general venous blood; they gave the same result, but much less pronounced.

These results were the subject of a memoir included in the Museum's tercentenary volume in 1935 (p. 283). I concluded in this memoir that the slowing down of normal oxidations in the venous blood leaving the parotid gland during its active state was due to the fact **that it had lost, in the organ it had passed through, part of a substance that serves for these oxidations and which, probably, would be an oxidizing ferment** (77, p. 285).

This conclusion, which approached the truth, did not contain it entirely. When I acquired knowledge of the colibacillary function, identified the fibrinferment which is at the same time the glycolytic ferment and the oxidizing ferment, then recognized the exis-

tence of colibacillary granulations in the blood, I had the key to the problem, through a microscopic examination of the saliva which also contains them.

As the considerable quantity of saliva which flows per minute (25 to 100 cc.) contains an enormous number of diastatic granulations and as it is impossible for the parotid gland of the ox to elaborate these granulations in a few seconds or even in a minute, they can therefore only come from the blood which contains them and which they leave with the liquid passing through the glandular cells to constitute saliva.

The slowing down of oxidations in the blood of the active parotid gland of the ox therefore results from the reduction in the number of colibacillary cocci it contains and which have passed into the saliva.

Proof of the accuracy of this explanation is provided by the following fact: if blood is centrifuged and filled into a sealed tube, it separates into three parts: one containing the red blood cells, above which is a whitish layer containing the white blood cells, platelets, and a large portion of the colibacillary cocci, and then, above this layer, the clear serum.

In this centrifuged blood, oxidation is completely eliminated, and it retains almost all of its oxygen, even after 48 hours in an oven at 37°C, while a similar control tube, but whose contents were shaken immediately after centrifugation, has lost it completely.

This cessation of oxidation in centrifuged blood results from the isolation of the colibacillary elements in a thin layer where they remain inactive, separated from the plasma and the red blood cells. It is therefore they who carry out these oxidation actions.

It is therefore not the digestive glands that give rise to the diastases, that is, to the formed elements that are their active elements; these elements are the colibacillary cocci of the blood that pass through the glands with the blood plasma.

The glandular cell intervenes only to make a selection among the substances in the blood plasma and determine the degree of alkalinity or acidity of the digestive juice, or to secrete substances that direct or multiply the diastatic activity of the colibacillary cocci in the digestive juice.

These actions explain why the same diastatic property, that of lipase, is slightly different in two digestive juices of the same organism, although the elements that exert this action have a common origin; thus, although both come from the lipase of the pancreatic juice, since bile activates it, but does not activate that of the gastric juice.

However, the diastatic properties of the blood colibacillary cocci are in principle preserved in the digestive juices. The gastric, pancreatic, and enteric juices possess proteolytic and lipase powers.

VII. The micrococcal colibacillary elements released by the blood into the digestive and glandular juices are the origin and source of staphylococci, streptococci, and pneumococci, which normally inhabit the nasal, oral, and pharyngeal cavities and the digestive tract, as well as of intestinal colibacillus, the latter by evolution from cocci.

This conclusion results from the following facts, established in this third volume:

1. The blood contains in the form of cocci, bacilli, platelets, and leukocytes (germinative masses) the elements of organic colibacillus;

2. In blood serum left to itself at room temperature, or even at a lower temperature, colibacillus cocci multiply and develop chains of streptococci at the same time as isolated elements or elements united into diplococci;

3. Pneumococcus is the colibacillary granulation surrounded by a thin layer of fibrin due to its coagulating power. Cultivated in a medium that does not contain fibrinogen, it loses its shell and no longer differs from streptococcus or staphylococcus;

4. The connective tissue contains the micrococcal elements of the colibacillus and its various germinative masses; these elements constitute a form of staphylococcus and confer coagulating power to the lymph that flows from the tissue after trauma;

5. All digestive juices contain the micrococcal elements of the organic colibacillus.

6. The micrococcal colibacillus elements of the blood possess the same diastatic properties as those of the digestive juices and they are the active agents of these properties.

Thus, if staphylococci, streptococci, and pneumococci are normally found in the nose, mouth, pharynx, and digestive tract, it is because they are continually poured there by the digestive juices, which themselves have received them from the blood, and it is because of this origin that they are devoid of virulence.

It is also from the blood that the colibacillary granulations that all other normal or pathological fluids contain come: pleural fluid, cerebrospinal pericardial fluid, peritoneal fluid, urine, milk, etc., granulations that have been identified with staphylococcus, streptococcus, pneumococcus; these, through evolution, can change form and take, for example, the bacillary form of colibacillus, as **Charrin** and **Veillon** were able to observe when, observing the existence of pneumococcus in a peritoneal exudate in a patient, they found, after death, only colibacillus, and were not in a position to think at that time that the second was the evolved form of the first.

At the beginning of the chapter on colibacillary function, we investigated the possible origin of the colibacillus contained in a pure state in the newborn's intestine immediately after birth and before any ingestion through the digestive tract. The preceding facts indicate that it is obviously through the digestive glands or simply the mucus glands of the stomach and intestine that the colibacillary elements in the blood enter. There, they are transformed into bacillary elements.

Colibacillosis is wrongly considered a urinary infection; however, it causes only minimal problems and, in some subjects, it even persists for a very long time or indefinitely without them being significantly bothered.

Colibacillosis is wrongly considered a urinary infection; however, it causes only minimal problems and, in some people, it persists for a very long time or indefinitely without any noticeable discomfort.

Colibacillosis is not an infection because the colibacillus always exists normally in the urine of every individual without any possible exception; it exists in the state of micrococcal granulation, the presence of which is easily observed in urine collected aseptically if, on a glass slide, two or three drops of urine are evaporated and the residue is stained with dahlia stain, or even better, if the operation is repeated several times on the same point of the slide. This is also easily observed by examining the centrifugation residue of urine collected aseptically whose density has been lowered by the addition of alcohol.

Urine always putrefies because colibacillary cocci multiply in it; they take the form of chains and, consequently, become streptococcus exactly as they do in blood serum left at room temperature; this common evolution is understandable since, in both cases, it is the same colibacillary granulation in or from the blood that evolves.

Left to itself at a temperature of 5 to 10°C, the slow evolution of the cocci results in the formation of a gelatinous mycelial veil made up of very long filaments identical to those that develop in oxalated plasma or in fibrinogen solutions; the gelatinous appearance of the veil results from the formation of a small quantity of inconsistent fibrin. Short, arched bacilli are born on the filaments.

These facts therefore establish that contamination of urine by colibacillary cocci is normal and inevitable, because they always pass through the kidney in certain quantities with the excreted urine.

Urinary colibacillosis therefore consists, not of an infection, but of an evolution, into the bacillary form, of the cocci form of the colibacillus that all urine normally contains. This transformation of cocci into bacilli obviously occurs because the urine becomes more

conducive to the evolution of cocci than in the usual state; urinary colibacillosis is not related to a kidney lesion, and, to make it disappear, it is sufficient to ingest one of the known and usual drugs whose effect is to modify the urine culture medium, particularly from the point of view of its reaction, and not to act on the colibacillus itself.

A specific anticolibacillary action is impossible in the body because, if a drug achieved it, it would suppress the colibacillary function, as important and indispensable as blood circulation and respiration and, consequently, would quickly cause death.

* * *

Like urine, milk normally contains a multitude of colibacillary cocci, which are those from the blood that has passed through the gland.

If we examine under a microscope a patch of spontaneously coagulated milk crushed between two glass slides, then stained with dahlia after heat fixation, we see an image similar to that of a crushed blood clot: an innumerable quantity of cocci, most of them emitting a short, thin, divaricate filament carrying fine zymospores and entangled with each other by this filament.

After two to three days, the coagulum contains larger cocci and a few more or less long colibacillary elements. Cultivated in glucose peptone broth, this coagulum gives rise to isolated cocci or grouped into diplococci, and to chains of streptococci, exactly as occurs in serum and urine left to their own devices, then to bacillary elements. Transferred to 2% peptone solution, the culture passes completely into the bacillary state.

This bacillus, which is **Eschrich's** *Bacillus lactis aerogenes*, is the organic colibacillus of the species from which milk comes.

The spontaneous coagulation of milk is therefore due to the micrococcal elements of milk coming from the blood. These blood granulations, which are the active elements of the fibrinferment, have the power to coagulate blood and milk, a power also possessed by streptococcus, staphylococcus, and pneumococcus, which are all micrococcal forms of colibacillus.

To conclude this question, we must observe that, knowing that urine always inevitably putrefies and that this putrefaction has been attributed to a micrococcus, it seems unacceptable that it has not been determined where this putrefaction and this micrococcus come from.

The responsibility obviously lies with the false dogma of the asepsis of living organisms, which forbade any researcher from thinking that a bacterium could normally exist in the blood and pass into the urine.

VIII. It is the micrococcal colibacillary elements of the blood (fibrin ferment) which, after injection of the blood serum of one animal into the blood of another species, develop in the latter the proteolytic ferment discovered by Abderhalden and which create the state of anaphylactic sensitization, as well as the precipitating power of the serum of the recipient animal towards the antigen serum.

This question is developed in the article "Anaphylaxis" (p. 331), so I refer the reader to it. I only examine it here to observe that, until now, the great error has been made in considering the state of anaphylaxis as caused by the injection of a foreign albuminoid substance. This fact may have appeared correct because, in every animal, there is a bacterial function and since the cocci which exercise this function exist in all regions of the body, it is impossible to extract from it an albuminoid substance which does not contain them.

In the injected albuminoid substance, it is not the latter that acts, but the cocci it contains; proof of this is provided by the fact that the solution of albuminoid substance (or the serum) being brought, for 15 to 30 minutes, to a temperature of 60 to 70°, the liquid loses its anaphylactic power, the vegetative and diastatic powers of the ferment being destroyed without the albuminoid substance being altered.

In **Lambling's** *Outline of Biochemistry*, we read this (p. 98):

All the diastases tested up to now have behaved like antigens, that is to say, when injected into an animal, they have caused the corresponding antidiastasis to appear in the serum of this animal.

It is not an antidiastasis that develops in the blood of the receiving animal; it is a substance that agglutinates the active elements of the diastase. Here is the mechanism of this phenomenon:

The injected diastase is made up of bacterial elements of the cocci form;

In the blood of the recipient animal, these multiply and, through the reactions they cause, particularly on fibrinogen and blood globulins, they develop a particular fibrin that agglutinates the injected and multiplied cocci in the form of germinal masses which, this time, will develop by producing bacterial filaments. This particular fibrin agglutinates the elements of the diastase in vitro and prevents them from exerting their action.

Any heterogeneous diastase is a heterogeneous virus, and its injection develops an experimental disease comprising an acute phase or bacterial evolution and a chronic phase or mycelial development.

It is during bacterial evolution that the agglutinating fibrin develops and it is the transition to the mycelial evolution phase that marks the moment when the state of anaphylactic sensitization is reached. The serum of the recipient animal, containing from this moment the agglutinating fibrin, added in very small quantities to the diastase mixed with a suitable fermentable liquid, it will agglutinate the elements and thus make it impossible for them to act; but it does not kill them, because, thus agglutinated, they can, if separated from the liquid, develop again in a culture broth.

* * *

IX. The fermentative part of the complement or alexin is the colibacillary granulation of the blood, that is, the fibrinferment

The blood serum contains the micrococcal elements of the colibacillus. They can be separated by vigorous and prolonged centrifugation. By dialysis, which eliminates the NaCl from the serum, the globulins are rendered insoluble and, by precipitating, carry with them the colibacillary granulations which are, as we saw previously, the fibrinferment.

What has been called thrombin, a mixture of serozyme and cytozyme, is in reality serum with its colibacillary granules. We know that thrombin ages very quickly and quickly loses its ability to coagulate oxalated plasma.

However, the precipitate of serum globulins, which has been called mittelstück (middle portion of complement), is inactive if used alone and only becomes active if mixed with the resalted liquid portion of the dialyzed serum (terminal portion of complement). This precipitate of serum globulins retains its activity for a fairly long time if it is left agglomerated without redissolving it in a NaCl solution, that is, in the state in which it is found after centrifugation. If, on the contrary, it is redissolved in salt water, it loses its activity fairly quickly. This is the well-known **Brandt** phenomenon.

This fairly rapid inactivation of the fermentative action of the colibacillus granulations in the blood serum is therefore identical, whether it is the coagulant action on fibrinogen or the complementary action of the alexin serum, because, in both cases, the active element is the same.

In addition to the colibacillary granules, the middle part of the complement contains globulins; as for the terminal part, containing all the other parts of the serum, it contains in particular the albumin complexes linked to cholesterol or its ethers. These appear to play the role of activating the colibacillary granules of the middle part, an action similar to the activation of pancreatic lipase by bile.

* * *

CONCLUSIONS

1. The active elements of the diastases are figured, bacterial elements, of micrococcal form. There is no diastase whose active elements are soluble, non-figure elements;

2. The active elements of all the diastases of the mammalian organism are the micrococcal granules of the colibacillus;

3. The granular elements of the diastases multiply by emitting a divaricate filament on which fine zymospores develop;

4. The granular elements of the diastases, cultivated in glucose peptone broth, evolve and transform into bacillary and mycelial elements, then into hyphomycetes;

5. In every living being, animal or plant, there is a bacterial function that is carried out by a single bacterial species and which carries out all the diastatic actions of its organism;

6. The micrococcal element, which is the active agent of the bacterial function of a living organism, possesses multiple diastatic properties;

7. The colibacillary granular elements of the blood, which are those that constitute the fibrinferment and the fermentative part of alexin, are the source of the granulations which, in all digestive juices, possess the diastatic power;

8. Staphylococcus, streptococcus, and pneumococcus, which normally inhabit the oral, nasal, and pharyngeal cavities and the digestive tract, are constantly discharged there by the digestive juices and mucus, which themselves received them from the blood, where they are the colibacillary cocci;

9. These are the colibacillary micrococcal elements of the blood serum which, after injection into the blood of another animal species, develop the proteolytic ferment discovered by **Abderhalden** and the state of anaphylactic sensitization;

10. Any granulation of living matter, from 0.2 to 0.8 or 1 micron, animated by Brownian movement, possesses diastatic power.

CHAPTER XI

STUDY OF DIPHTHERIA

THE DIPHTHERIA VIRUS IS THE LIVING MATTER OF CEREALS (WHEAT FLOUR, BARLEY, AND RYE) EITHER IN ITS NORMAL FORM OR IN ITS TRANSFORMATION FORMS WHICH CONSTITUTE THE SO-CALLED CRYPTOGAMIC DISEASES OF THESE PLANTS

Cause of Diphtheria

When the living matter of plants is affected by physiological misery, either by defective cultural conditions, or by unfavorable climatic or meteorological conditions, or even by senility, it tends to lose or loses the organized form, takes on new forms and passes into the purely mycelial state; it then takes on such varied aspects that thousands have been described and classified as genera and species of lower parasitic fungi, Basidomycetes, Ascomycetes or Hyphomycetes which, in reality, are nothing other than autogenous formations resulting from the direct transformation of the living matter of plants, a fact already demonstrated since 1926 in the first volume of this work. It is the extraordinarily developed property of polymorphism characteristic of living matter which allows it to metamorphose in this way under multiple aspects in the same plant species.

When fragments of living matter are isolated from the original plant and kept moist, they suffer the same fate; they quickly lose their organized form, and their living matter develops by going through the phases of mycobacterial evolution that I described in the first volume of this work, thus giving rise to either bacterial or mycelial elements.

When the vitality of the living matter of the grain of cereals, reduced to flour, has not been destroyed by heat, it undergoes these transformations directly; if this flour has been used to make a food, particularly porridge intended for a child, the particles of this food that remain adherent to the tonsils or other points of the mouth or pharynx undergo a rapid transformation and, in 24 or 48 hours, constitute a veil or mycelial network that is the beginning of the false diphtheria membrane.

This transformation occurs all the more quickly and easily because flours contain spores or conidia of the different conidial forms of the fungi considered to cause cereal diseases, conidia that exist inside or on the surface of the grain (wheat bunt, *Cladosporium herbarum,* etc.) or on the surface or under the epidermis of the glumes of the stem and leaves (cereal smut, rusts, *Cladosporium,* etc.). These spores or conidia, released by threshing, are carried along with the grain and then inevitably mixed with the flour.

They are more resistant to heat than untransformed living matter and, like it, give rise to mycelial filaments.

This is the very simple cause of the development of diphtheria and the false membrane. This cause is not the diphtheria bacillus directly, because flour does not contain it;

the bacillus only appears in the false membrane after the formation of the mycelium of which it is a product and not the formative agent.

In fact, if the diphtheria bacillus is spread on the surface of the bare germ of a rabbit's ear, it forms a false membrane and, coming from the pharynx of one individual, it can contaminate another; but these facts do not constitute proof that it is the original diphtheria virus, the one which, practically, is the primary infecting agent, the cause of the development of the false membrane, because one of the first demonstrations made in this work was the reversibility of the transformation of living matter: the elements of a bacterial culture are easily transformed into Hyphomycete, but the latter is just as easily returned to the bacterial state.

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The proof that the living matter of cereal grains is indeed the original diphtheria virus lies in the fact that by cultivating the living substance of a healthy, normal, flamed barley grain, *Cladosporium herbarum* is obtained on a solid medium (agar) and, in a liquid medium, the diphtheria bacillus. Then, even more demonstratively, by inoculating the conidia of *Cladosporium herbarum* from barley in broth, mycelial colonies are obtained on which the diphtheria bacilli develop (see plate 64).

The fact that it is not uncommon for the false diphtheria membrane not to contain bacilli adds to this evidence, which, moreover, is superfluous, because the formation of the diphtheria bacillus, by a small particle of normal, healthy cereal flour, coming from a singeing grain, is enough to impose, beyond any possible dispute, the new notion that cereals are indeed the original source of the diphtheria virus and that their living matter is indeed this virus itself, the primitive infecting virus.

Diphtheria is not contracted by contagion but by ingestion, through he digestive tract, of cereal flour, which is the virus.

This explains why it is mainly children who are affected by diphtheria: it is because their diet, particularly in infancy, consists mainly of cereal flour porridge and infected cow's milk.

This implies that young children, who are breastfed exclusively by their mothers, must remain free from diphtheria, measles, scarlet fever, and chickenpox, as long as they have no contact with cereal flours, either directly or through their mothers' hands, and as long as they do not drink cow's milk, which is very often contaminated by elements of

the original virus contained in the litter manure that soils the limbs, flanks, and udders of animals; the litter, made up of wheat, barley, rye, and oat straw, in fact contains the same elements of the original virus as the grain.

This also answers the very just objection of those who, not believing in contagion in epidemics, said: And how did the first diphtheria patient in the epidemic contract the disease? Now we know it was by eating undercooked wheat, barley, or rye flour or by drinking milk contaminated by cow litter. It can be added that it is not only the first, but all the diphtheria patients in the epidemic who contract the disease in this way and not by contagion, which has been considerably exaggerated due to total ignorance of the nature and origin of the virus.

Explanation of the development and location of diphtheria epidemics as well as the influence of meteorological factors on this development.

The location, in a single district of a city, is naturally explained as follows: the flours that a city consumes come from the most diverse regions of France. The cereals that supplied them were cultivated, some in dry soils, others in very humid soils, some in regions where the climate is dry, or in regions where it is rainy.

So-called parasitic diseases of cereals are directly dependent on:

1. Meteorological conditions (humidity, heat);

2. Cultural conditions (poor, humid, marshy soil, etc.);

3. The origin of the seeds.

The cereals of one region can therefore be free from these diseases while they are very affected in others. In these regions, the grains and consequently the flours are heavily laden with conidia of smut,

Cladosporium, wheat bunt, rusts, etc. These conidia are more resistant to heat than normal living matter and, in any case, give rise to the false membrane more quickly than the latter.

Flour rich in spores of smut and *Cladosporium herbarum* that is received by a merchant and distributed only within a restricted area will cause an epidemic of diphtheria, while the other districts of the city will remain unaffected.

This also explains the influence of meteorological conditions on epidemics in general. At the end of a stormy, hot, rainy year, favorable to the development of *Cladosporium*, smut, and rusts, and during the following year, there will be many more cases of diphtheria

than after a dry, non-stormy year, because the flours put into circulation will be more laden with pathogenic elements.
Diphtheria has developed so far because foods containing cereal flours have not been subjected to prolonged boiling to destroy the vitality of the living matter, that is, its power of mycobacterial evolution. It mainly affects children because it is they, in particular, whose diet includes, from the age of six months and for years afterward, the daily use of cereal flours in the form of porridge.

These are particularly dangerous when made with milk because, firstly, the latter is not boiled, most often, for more than a minute to prevent it from acquiring a bad taste by carbonizing on contact with the bottom of the pan and because, secondly, it is often contaminated by fragments of litter manure, the latter being made of cereal straw soiled by the same anthrax conidia, *Cladosporium*, rusts, etc., as the grain.

As for non-milk porridges, merchants who sell flour intended for children's food recommend boiling for five minutes, which is insufficient.

The remedy is therefore very simple: Eliminate the vitality of the living matter in cereal flours by applying sufficient heat in terms of degree and duration, and diphtheria will no longer be seen. It will disappear from the list of diseases without the need for preventive inoculations with toxoid, that is, a live virus which, once introduced into the body, risks causing various disorders; the effectiveness of this toxoid will, moreover, be examined later.

More generally, subject all children's foods to this sufficient heat and measles, scarlet fever, chickenpox, diphtheria, poliomyelitis will disappear from the list of their diseases. Indeed, we will see later that the viruses of these diseases are, like that of diphtheria, the living matter of cereals.

One of the forms in which the virus develops the false diphtheria membrane is often the Cladosporium herbarum of barley, wheat, and rye.

In the first volume of this work, I had already established twenty years ago (p. 498, 1st vol., 1926) that the diphtheria virus is the living matter of barley, which I called its organic mold, and that the false diphtheria membrane was made up of a mold that I believed to be of the *Citromyces* or *Penicilium* type.

The very prolonged study to which I subsequently devoted myself, the results of which I am about to present, has demonstrated that my determination of the nature and original source of the diphtheria virus was correct but nevertheless incomplete because wheat, rye, and even other cereals can, like barley, develop the diphtheria false membrane. The living matter of these cereals, in its organized form, is the original virus; reduced

to minute fragments in flour, it alone is sufficient to cause diphtheria and the formation of the false membrane.
The mycelium that constitutes it develops there in the form of *Cladosporium herbarum* or other hyphomycetes; *Cladosoprium* can develop directly in cereal crops, on the grain, the stem and the leaves of the plants, under the influence of unfavorable weather conditions, prolonged rain, and excessive heat.

The conidia of *Cladosporium herbarum* are released by threshing and carried along with the grain, which may itself contain them on its surface and under its epicarp. The presence of these conidia in flour considerably increases the possibility of the development of the false membrane, which healthy, normal living matter is nevertheless capable of developing on its own.

The tonsils developed by Cladosporium herbarum in other plants.

The development of *Cladosporium herbarum* on living plants has been observed on more than 150 other plant species, including potatoes, carrots, cabbage, beans, eggplant, salsify, fennel, spinach, etc. It is even almost certain that it can develop on almost all plants because, along with *Monilia* and *Oidium*, it is one of the best-known forms taken by living plant matter.

The living matter of some of these latter food plants, when insufficiently cooked, is therefore capable of developing false membranes of the *Cladosporium herbarum* type, which can give rise either to bacillary forms similar to, if not identical to, the diphtheria bacillus, or to pseudodiphtheria bacilli. Other food species may give rise only to a pultaceous coating or only to an inconspicuous bacterial culture.

These food plants therefore develop tonsillitis of various forms in the pharynx. It is especially plants eaten raw or almost raw that can develop them. These tonsillitis are studied later. But let us note, from now on, that only the systematic search for the virus and its original source can succeed in revealing these tonsillitis of such varied origins which, often, are only the original lesion and the gateway for the virus of a disease that medicine does not yet understand.

These preliminary notions having been explained, let us now move on to the demonstrations.

We will study the following points in turn:

1. Diphtheria false membranes are constituted by the mycelium of a hyphomycete fungus;

2. Cladosporium herbarum is very often the hyphomycete that constitutes the diphtheria false membrane;

3. The diphtheria bacillus of the false membranes is not the initial infecting virus but a secondary formation of the mycelium that constitutes them.

A) By broth or agar culture of *Cladosporium herbarum*, the diphtheria bacillus is reproduced.

B) Broth culture of the identified diphtheria bacillus can develop a mycelial veil of *Cladosporium*.

C) Broth culture of barley *Cladosporium* gives rise to distinct submerged, spherical, and radiating mycelial colonies and, on the mycelium branches, to characteristic diphtheria bacilli.

D) Culture of barley *Cladosporium* on agar gives rise to the diphtheria bacillus and its involution forms;

4. The connection between the diphtheria bacillus and the mycelium of the false membrane is established by the reproduction, by the diphtheria bacillus, of a false membrane on the rabbit's ear, or of a false membrane in vitro culture on broth;

5. The living matter of externally sanitized wheat, barley, and rye grains, cultivated in broth, causes the formation of the diphtheria bacillus in 24 or 48 hours and, when inoculated directly onto the rabbit's ear, causes the formation of a false diphtheria membrane;

6. False membranes can be developed by other conidial forms of barley, wheat, and rye and, more rarely, by *Cladosporium herbarum* of other plants, or by other conidial forms originating from them.

7. The various original sources of the diphtheria virus; forms in which it causes contamination;

8. The different conidial forms of the living matter of the same food can cause different diseases. It is the conidial form that creates the character of each disease. Two conidial forms of the same food species developed together can cause two simultaneous diseases;

9. Is diphtheria toxoid made with a culture from wheat, barley, or rye diphtheria? Does inoculation with toxoid from barley diphtheria culture protect against diphtheria caused by wheat or rye, and does wheat diphtheria toxoid protect against barley diphtheria?

10. Antidiphtheria serum;

11. Avoiding the disease is better than immunizing against it. The fight against diphtheria must consist of avoiding the ingestion of the live virus and not of inoculating this virus, even if attenuated.

* * *

1. Diphtheria false membranes are made up of the mycelium of a hyphomycete fungus.

To have before one's eyes the demonstration of this fact, it is sufficient to dissociate, on a glass slide, a fragment of false diphtheria membrane and to examine it under the microscope after having colored it with dahlia or any other coloring. Figures 1 to 5 of plate 52 are photographs of fragments of this dissociation. In figure 1 one sees, at a low magnification, a group of germinative masses emitting large mycelial filaments and, in figure 2, other long filaments of varied diameters. It is the same in figures 3, 4, 5. The same demonstration is given by plate 57, figure 4, showing large mycelial branches of a dissociated false diphtheria membrane, as well as by plates 53, 54, 55. All these false membranes were taken from diphtheria children at the Trousseau hospital.

Other evidence demonstrates the constitution of false membranes by the mycelium of a hyphomycete. These are:

1. The transformation of a culture of diphtheria bacillus into a mycelial veil bearing the conidia of the mold (fig. 1, 2 pl. 57).

2. The formation on the ear of a rabbit, infected by a verified diphtheria culture, of a false membrane constituted by the mycelium of a mold (fig. 1, 2, 3, 4 pl. 58). This evidence is presented later.

2. Cladosporium herbarum is, very often, the conidial form of the mold constituting the diphtheria false membranes; these can also be constituted by other conidial forms, notably by the Citromyces and Aspergillus forms.

Microscopic examination of the dissociated false membranes shows that they contain a considerable quantity of generally oval or more elongated bodies which are a production of the mycelium to which they are often seen attached by a pedicle which is their conidiophore; these are the conidia of the hyphomycete which constitute the false membrane and which is very often *Cladosporium herbarum*; here is some proof:

1. By seeding the skin of the inner surface of a rabbit's ear, deprived of its epidermis by the action of a small blister measuring 4 or 5 centimeters by 3 centimeters, with conidia of *Cladosporium herbarum* from barley, wheat, or rye, a false membrane is formed who-

se constitution, mycelium, conidia, and diphtheria bacilli it forms are identical to those of a human diphtheria false membrane. This is established by Figures 1 and 2 of Plate 60, showing the elements of a false membrane, developed by seeding the conidia of *Cladosporium herbarum* from barley on a rabbit's ear **(1)**. Comparison of these two figures with Figure 4 of Plate 57, photographs of a fragment of human diphtheria false membrane, shows the identity of the form of the mycelium in both cases.

The rabbit ear false membrane, photographed in Figures 1 and 2 of Plate 60, was devoid of bacterial elements. When inoculated in broth, it gave rise to a bacterial culture, the veil of which is photographed in Figure 4.

Figure 3 shows the veil elements from the subculture of the previous original culture. Both contain short and medium diphtheria bacillus and a larger bacillary form, which is also a form of diphtheria bacillus, which will be discussed later.

Figures 5 and 6 are smears of another false membrane, developed on the ear of another rabbit inoculated with conidia of the same strain of *Cladosporium* from barley as for the previous false membrane. These two figures show the diphtheria bacilli of the false membranes with their characteristic shape;

2. Figures 2 and 5 of Plate 52 show the various dimensions and shapes of the mycelium of a diphtheria false membrane from humans. Figure 5 in particular shows the very special appearance that certain mycelial filaments take on when they have lost most of their basophilic protoplasm.

This aspect is found, identically, in certain mycelial filaments of cultures obtained by inoculation in broth of conidia of the *Cladosporium* of barley; such a culture produces a mycelial veil whose elements are photographed in figures 1 to 4 of plate 65. We see, in figure 3 this mycelium fragmented into large bacilli of appearance and dimensions identical to those of figure 5 plate 52 of a false diphtheria membrane;

3. If we look for the various forms of conidia in the diphtheria false membranes, we find both the great variation characteristic of *Cladosporium herbarum* and, above all, certain forms that surely characterize this hyphomycete. This operation was carried out in plate 53, which shows the elements of a human diphtheria false membrane. The conidia are generally oval, but others, such as those in figures 5 to 10, are characteristic of *Cladosporium herbarum*.

We will compare the forms of the conidia of the diphtheria false membrane, which is the subject of this plate 53, with those of the conidia:

(1) *A rubberized cloth cap, completely protecting the ear up to the skull, prevents infection of the false membrane by external germs.*

1. Of a *Cladosporium herbarum* obtained directly by culture on agar from the crushed product of a flamed wheat grain (pl. 61);

2. Of another *Cladosporium* obtained by culture on rye flour agar (pl. 62);

3. Finally, a third *Cladosporium* obtained by culture on agar of the pulped lung tissue of a guinea pig inoculated with the emulsion of a fragment of crushed false diphtheria membrane (pl. 54).

This comparison shows us the identity of form:

A) Of the elongated and asymmetrical conidium of figure 5 plate 53 with the conidia asymmetrical J5, G9, figure 2 plate 61; G5 figure 1, plate 56 (Identification by the grid);

B) Of the conidia 16 (fig. 1) E2 (fig. 3). Those of figures 6, 7 and 9 plate 53 with the conidia H2 figure 2, and F2 figure 5, plate 54; E14, E12 figure 1, plate 56;

C) Conidium Figure 8 Plate 53 with conidium CI5 Figure 1 and even C8 Figure 2 Plate 54. As for the oval-shaped conidia, we find very variable shapes with a very short or more elongated major axis, both in the false membranes, as shown in Plate 53, and on the mycelium of *Cladosporium* in pure culture. This great diversity of conidial shape is characteristic of this genus of hyphomycetes. **Rabenhorst** describes the conidia thus in the diagnosis of *Cladosporium herbarum* (69 T.7 p. 801). This diagnosis is literally translated as follows **(1)**:

Conidia terminal in appearance by growth of the end of the conidiophore, lateral and attached on small, non-pointed (truncated) protuberances isolated or sometimes growing in chains (especially after falling) of the most varied forms, oblong, oval, and then, most often, unicellular, or elongated cylindrical, ellipsoid and then with one-four partitions, dirty brown and olive green, with partitions as if laced with their membrane grainy or provided with prickles, of very different size and length.

In summary, what characterizes the conidia of Cladosporium herbarum is the extreme variability of their shapes and dimensions. Some of the elongated forms are characteristic, particularly the asymmetrical forms, and we have just seen that they exist in diphtheria membranes. The conidia that the mycelium of these produces in considerable quantity are therefore, without the slightest doubt, those of *Cladosporium herbarum*;

(1) *Here is the text of this diagnosis:‘’*Könidien enständig, dürch fortwachsend der Trägerspitze scheinbar seitenständig ûnd aüf stümpfen Höcker-chen aŭfsitzend, einzeln oder bisweilen zü Ketten sprossend (namentlich nach dem Abfallen), von mannigfachster Form, länglich, eiformig und dann meist einzellig, oder zylindrish, länglich, ellipsoïdisch ūnd dann mit 1-4 Scheide-wänden, schmützig braün ünd olivengrün, anden Wänden etwas eingeschnürt, mit sein Körniger oder stachliger Membran, von sehr verschiedener Dicke ünd Länge. >*

4. *Cladosporium herbarum* can be reproduced by culture on solid medium of diphtheria false membranes or by culture of the pulp of the liver, lung, and spleen of a guinea pig to which an emulsion of crushed false membrane has been inoculated under the skin. Pl. 54 (figs. 1, 2, 5) and 56 (figs. 1 to 7) show the *Cladosporium* thus obtained from two diphtheria false membranes.

For the diphtheria false membrane in plate 56, I obtained two distinct forms of *Cladosporium herbarum* by injecting the guinea pig, not with the false membrane itself, but with a broth culture that I obtained from it; one is the usual type showing all the variability of conidial shape (figs. 1, 3, 7 pl. 56) the other a special type producing conidia that are regularly oval and considerably larger than those of the previous form (figs. 2, 4, 6 pl. 56) with a few rare elongated and even larger conidia. The mycelium of this second type also shows clear differences from that of the first type. I also obtained these two types of *Cladosporium herbarum* by direct culture on agar of flamed and crushed wheat grains; we will note the perfect identity of these two types with those obtained from a false diphtheria membrane plate 56 by comparing them, either to the usual type of *Cladosporium herbarum* of wheat (fig. 1, 2, 4 pl. 61 J), or to its type with large oval conidia (fig. 1 to 7 pl. 68). But, in this last type obtained from wheat, the oval conidia develop and elongate, forming one to four partitions, a fact which brings them within the framework of the diagnosis of *Cladosporium herbarum* of **Rabenhorst**.

I recall here that I concluded from my observations and research contained in the first volume of this work that all the forms of abnormal production of plants currently classified as parasitic fungi and agents of certain cryptogamic diseases are in reality exclusively autogenous productions of the living matter of the plant itself.

All the observations I have made since 1926 have only confirmed the accuracy of this conclusion, and the observations in this chapter demonstrating the production of *Cladosporium herbarum* in vitro by the substance of healthy cereal grains, rendered aseptic by singeing, also fully confirmed it.

A new demonstration of these facts concerning mycorrhizae and the tuberization of plants will be found on page 158 of this volume.

All these abnormal productions, such as *Cladosporium,* Botrytis, smut, rust, etc., are due to abnormal vegetation of the living matter of the plant caused either by cultural conditions unsuitable for the species, by unfavorable atmospheric conditions, or by the plant's state of senility.

The fact that, experimentally, a healthy plant can be infected with conidia from a pathological formation on another plant cannot in any way invalidate the conclusion that the pathological formation develops autonomously and spontaneously.

I recall these facts necessarily to respond in advance to the objection that one could make by claiming that the false diphtheria membrane is formed by conidia of *Cladosporium* contaminating the surface of cereal grains (wheat, barley, rye) and mixed with the flour and not by the living matter of the grain. Here, moreover, is the formal proof that the living matter of wheat can develop the false diphtheria membrane.

1. Healthy cereal grain (wheat, barley, rye) quickly flamed and immediately plunged into sterile cold water, then crushed and applied to the bare dermis of the inner surface of the rabbit's ear, develops a false membrane identical to the false diphtheria membranes, containing the typical conidia and, in addition, characteristic diphtheria bacilli.

Plate 67 shows in figs. 5 to 8, 9 to 13, 14, 15, smears of three false membranes from three rabbits thus treated with barley grain; the bacillary forms of these smears are characteristic and identical to those of the diphtheria bacilli of the false membranes of humans.

2. The broth culture of the diphtheria bacillus quickly forms a veil which can become completely mycelial. Figs. 1 and 2 of plate 57 show the transformation into Cladosporium of a diphtheria broth culture; the conidia are oval when viewed in profile, round when viewed along the axis of their greatest diameter; some, in fig. 2, are thinner. Some are still attached, by their fine formative branches, to the mycelial branches. This is therefore a veil of *Cladosporium*, and, very precisely, an artificial false diphtheria membrane formed in vitro by a culture of diphtheria bacillus.

In this same plate 57, figures 2, 3, and 4 are photographs of two dissociated fragments of a human false diphtheria membrane. Comparison of the elements of the artificial false membrane in figures 1 and 2 of plate 57 with those of the human false diphtheria membrane shows that they do not differ in any way.

From these different proofs, it results with certainty that the diphtheria false membrane is constituted by the mycelium of *Cladosporium herbarum* of a plant species. It would seem, according to the results of the inoculation of the guinea pig with the false membrane of man of plate 56, that one of the two forms of *Cladosporium* obtained (fig. 2, 4, 6) relates to wheat; one cannot affirm this because it is possible, even probable, that the *Cladosporium* of barley and that of rye, can take this same form.

The distinction between the *Cladosporium herbarum* produced by these three species of cereals does not seem possible; they appear morphologically identical.

3. The diphtheria bacillus of the false membranes is not the initial infecting virus, but a secondary formation of the mycelium that constitutes them and which is itself developed directly by the living matter of the cereal grain in the form of a hyphomycete, which is the true initial virus of diphtheria, or by the conidia of Cladosporium contained in cereal flours.

The diphtheria bacillus is not a preformed virus, which would develop here and there on various organic materials. Nor is it a bacterial species since, as we shall see, it is born on the mycelial branches of *Cladosporium herbarum*, of which it is a part, an emanation, branches which, moreover, also give rise to other bacterial forms very different from **Loefler's** bacillus and which can reproduce diphtheria just as well as the latter.

The diphtheria bacillus is not practically or actually the infecting agent of diphtheria. The real, initial, and direct infecting agent is the living matter of barley, wheat, and rye, contained in their seeds, or the conidia and spores of smuts, rusts, *Cladosporium*, etc., released from the plant by threshing and mixed with the grains, then with the flour they contaminate. In the first volume of this work, it is demonstrated that these conidia and spores are themselves autogenous productions of the living matter of the plant.

The diphtheria bacillus, born on the mycelium of the false membrane, therefore forms after it and, consequently, subsequent to the infection. It is therefore not the infecting agent. Here are the facts that demonstrate these assertions:

A) By broth or agar culture of *Cladosporium herbarum* conidia, the diphtheria bacillus is reproduced with its typical form and all its characteristics. Several cases arise in the evolution of this culture.

The broth culture develops an exclusively bacterial veil. Such a veil from a three-day culture of *Cladosporium* conidia from barley is photographed in Fig. 5, Plate 65. The granulations of the bacilli, particularly the polar granulations, can be seen in region F11.These bacilli take on the Gram.

Figure 6 shows short, medium, or long bacilli (depending on their state of segmentation) from another veil of a 6-day culture of the same strain of *Cladosporium.*

Figure 7 shows the elements of a 2-day agar culture, originating from an inoculation by the veil of a broth culture identical to the two previous ones.

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It shows short, medium, or long diphtheria bacilli, including typical bacilli that have already lost their basophilic material, except that of the two polar bodies. These bacilli take on Gram staining.

B) The broth culture of Cladosporium from barley can develop a mycelial and bacterial veil with large ramified branches.

Figure 1 of Plate 63 shows a photograph of such a veil from a 3-day culture; The largest mycelial branches in the figure give rise either to fine diphtheria bacilli (I. 9) or to the form of broad diphtheria bacilli (I. 6, I. 8). This broad form that is observed in most diphtheria membranes, and in the cultures obtained from them, is a diphtheria bacillus in the same way as the finer bacilli and not, as is claimed, a microbe of another nature from a microbial association. The same is true of other much larger mycelial segmentation elements, for example the large granular elements in figures 2 and 5 of plate 52 (smears of false diphtheria membranes) which are also diphtheria bacilli in the same way as the fine bacilli.

This fact of the production by a single mycelial trunk and its ramifications of segmental elements of all sizes, from 1/4 or 1/3 of a micron to 5 or 6 microns in width and even more, has already been reported and demonstrated in the first volume of this work.

C) Broth culture of Cladosporium conidia can also give rise to distinct submerged, spherical, and radiating colonies.

Figure 1, plate 64, is a photograph of two radiating colonies from a 5-day broth culture inoculated with conidia of *Cladosporium herbarum* from barley; Figures 2, 3, 4, and 6 are photographs of radiating mycelial branches that constitute these colonies; Figures 5 and 7 show clusters of bacilli floating in the culture and consisting of the most characteristic diphtheria bacilli that can be seen.

In these figures, we see diphtheria bacilli emerging at multiple points on small conical projections of the mycelium. In Figure 8 (I. 8) we see the short diphtheria bacilli (products of segmentation into two parts of the long bacilli) which have lost their basophilic matter, except for their two polar bodies.

The long bacilli have four basophilic granules, sometimes six; they segment into two short bacilli when they have four granules, into three if they have six, and the two granules of each short bacillus are its polar bodies.

These mycelial branches, with the bacilli that emanate from them, are the exact in vitro reproduction of the constitution of the false diphtheria membrane by the conidia of *Cladosporium herbarum* of barley, wheat, and rye, the original sources of diphtheria.

The original diphtheria virus is therefore not actually a microbe, but the living matter of food grains, barley, wheat, rye. **Loefler's** bacillus is only one of the bacterial products of the virus, which gives rise to other conidial forms of hyphomycetes, to other bacillary types, and to cocci of various sizes.

These very diverse bacterial forms whose presence has been observed in diphtheria false membranes are therefore not, as is believed, microbial associations of different species but varied bacterial forms produced on the mycelium of the same hyphomycete. Pl. 64 therefore provides the most peremptory proof that can be given:

1. That the original source of diphtheria is the grain of food grains;

2. That the diphtheria false membrane is constituted by the mycelium of a hyphomycete formed by the living matter of the grain, or by the conidia of its degenerative forms, those of cereal smut, for example;

3. That the diphtheria bacillus is not a distinct virus, a distinct bacterial species, vegetating on various organic media, but that it is only an element which appears secondarily on the mycelial branches which constitute the false membrane and, after the beginning of their formation, by the living matter of cereal flour or by the conidia of one of its conidial forms, *Cladosporium herbarum* for example.

The bacillus can reconstitute a false membrane simply by the development of its culture, a fact that will be demonstrated a few lines later, but which does not authorize the conclusion that diphtheria develops by contagion and transport of the bacillus from one individual to another. We will see later that it is completely useless to invoke contagion to explain diphtheria infection, given that wheat or barley flour, a daily food of young children, can develop the diphtheria bacillus in 24 hours, a fact that will also be demonstrated later.

D) Culturing barley Cladosporium on agar gives rise to the diphtheria bacillus and its characteristic involution forms.

Alongside the olive-green turf of *Cladosporium herbarum* on agar, a viscous material develops, which spreads out and contains the diphtheria bacillus. Figures 8 to 12 of Plate 65 show the bacterial elements developed in this material; these are long, medium, and short diphtheria bacilli and the elements called involution forms of the diphtheria bacillus. These characteristic involution forms are seen in all five figures 8 to 12.

After these four demonstrations, there can be no doubt about the exact nature of the real diphtheria virus, about the fact that **Loefler's** bacillus is not the initial infecting agent, the original virus of this disease, and that it is only one of the multiple, moreover inconstant, bacterial productions of the mycelium that constitutes the false diphtheria membrane.

4. The connection that exists between the diphtheria bacillus and the mycelium of the false diphtheria membrane is also established by the reproduction, by the diphtheria bacillus, of a false membrane on the rabbit's ear or of an artificial false membrane "in vitro."

A culture of **Loefler's** bacillus in broth or on agar is capable of artificially reproducing the false diphtheria membrane and of spontaneously transforming into one of the hyphomycetes that constitute it, *Cladosporium, Citromyces, Aspergillus*, etc.

One of my verified cultures of diphtheria bacillus spontaneously produced the entirely mycelial veil whose mycelium and conidia are photographed in Figures 1 and 2 of Plate 57. I recall that a bacteriology laboratory made available to me a culture of diphtheria bacillus by puncture on gelatin which showed spontaneous transformation into a *Citromyces*, a photograph of which can be found in the first volume of this work.

Furthermore, when inoculated onto the bare dermis of the inner surface of a rabbit's ear, the diphtheria bacillus forms a false membrane there. Plate 58 shows the constituent elements of such a false membrane produced by inoculation of the diphtheria culture photographed in Figure 5; Figures 7 and 8 are photographs of smears of the false membrane, and Figure 6 is a photograph of its broth culture.

These photographs demonstrate the characteristic change in shape of the diphtheria bacillus when it develops in the false membranes and when it is transferred from them into broth.

Pl. 59 also shows in Figures 2 and 4 the bacterial forms of the oozing fluid from a false membrane produced on the rabbit's ear by a controlled culture of diphtheria bacillus of human origin (Figs. 1, 3, Pl. 59). Figures 5 and 6 are smears of the false membrane showing, as in the previous case, the special form of the diphtheria bacillus in the false membrane, while in the oozing fluid (Figs. 2, 4) many elements have already resumed their normal form in culture, some of them showing so-called involution forms.

Here, the diphtheria bacillus is, either in vitro or on the rabbit's ear, the cause of the formation of the false membrane. But it should not be deduced from this that the diphtheria bacillus is, practically and normally, the causative agent of diphtheria, that is, the primary and usual infecting agent.

It is capable, like all other products that emanate from the real virus, which is the living matter of the grain of food cereals, of reproducing one of its primitive forms, for example its *Cladosporium* form, which is much closer to the living matter of the plant than the bacillus itself.

This does not mean that it is the usual infecting agent. The proof that it is not the original diphtheria virus is that the living matter of the grain of food cereals, which is this

original virus, is capable not only of itself directly developing the false diphtheria membrane on the rabbit's ear, but also of developing a characteristic diphtheria culture in 24 hours when inoculated in broth in vitro. Here are the facts.

5. The living matter of externally sanitized barley, wheat, or rye grains, cultivated in broth, causes the formation of the diphtheria bacillus in 24 to 48 hours and, when inoculated onto the rabbit's ear, determines the formation of the false diphtheria membrane.

A) Here is how I practiced the culture of the living matter of grains in broth: A grain of wheat, barley, or rye is removed from a healthy, normal ear, it is passed quickly through the flame of a Bunsen burner and immediately thrown into a little sterile cold water, then quickly ground in a sterile mortar. The flour obtained is inoculated into glucose peptone broth.

Plate 66 contains a photograph of the elements of the cultures obtained. Figures 1 and 2 concern barley; they show the elements of the veil of a culture.

48 hours of the grain grinding product. The long, large black mass on the right of Figure 2 is a group of germinative masses that give rise to long mycelial filaments such as those in Figure 1.

These two figures show long, medium, and short bacilli, depending on the degree of segmentation. They are granular and take Gram stains. Figures 3 and 4 show the elements of an agar culture of conidia of *Cladosporium* from barley and are included only as terms of comparison.

Figures 5, 6, 7, 8, and 9 concern rye. The living matter of healthy, sanitized, and ground rye grain forms a veil in broth in 24 hours. Figure 5 shows the elements of this 24-hour veil, and Figure 6, after 72 hours. These are long, medium, or short bacilli depending on the state of segmentation; they are granular and take Gram stains.

Figure 7 shows another 4-day culture; we see the formation of germinative masses, budding masses, one of which, in D. 4, D. 5, is in the process of forming; it is a small mass of achromatic hyaline matter already containing a little basophilic matter and which will be added by the adhesion and fusion of bacillary elements.

Figures 8 and 9 show: Figure 9 a 4-day broth culture, Figure 8 a 17-day culture. We see medium and short bacillary forms and the thick bacillary type. In addition, we see short, transparent elements with two polar globules which are certainly the segmented elements of the thick bacillary form and have lost most of their basophilic matter.

Figures 10 and 11, Plate 66, show the elements of broth cultures from the grinding product of a healthy wheat grain, externally sanitized. Figure 10 shows the elements of the

forming veil of a 24-hour broth culture, Figure 11 that of the veil of an 8-day culture. These are long, medium, or short, granular, Gram-positive bacilli.

Figure 12 shows the culture of flour from a sanitized oat grain, Fig. 13 the culture of the living matter from the oat leaf.

B) Culture of the living matter from the grains on the rabbit's ear.

Placed on the bare dermis of a rabbit's ear, the ground grain develops the usual false membrane with its mycelium, conidia, and diphtheria bacilli having the characteristic shape they take in false membranes

Plate 67 shows the forms of the bacillary elements of the false membranes developed on the ears of three rabbits by the product of grinding sanitized barley grains. Figures 5 to 8 relate to the first, Figures 9 to 13 to the second, Figures 14 and 15 to the third. These bacillary forms, as well as the involution forms, developed by the living matter of barley, prove to be identical to those of the diphtheria false membranes of humans; like them, they take on Gram.

Figures 1 to 5 are smears of a false membrane developed on the ear of the rabbit by the conidia of *Cladosporium* of barley; they show the same identity of bacillary forms.

Thus, in summary, the living matter of barley, wheat, and rye grains, cultivated in broth, produces a veil of the typical diphtheria bacillus within 24 hours, and, like a culture of diphtheria bacillus of human origin or like the conidia of *Cladosporium herbarum*, this living matter, inoculated onto the rabbit's ear, develops a typical false diphtheria membrane.

This living matter is therefore both the diphtheria virus and its original source.

There is therefore no need to invoke the existence of diphtheria cultures spreading here and there on organic matter, cultures whose existence no one has ever observed anywhere.

On the other hand, humans, and especially children, swallow foods containing barley, wheat, or rye flours every day, and it is enough for tiny particles of these flours to remain on the tonsils or in certain parts of the pharynx, or to be projected into the nasal cavities or larynx during coughing, for them to give rise to a diphtheria mycobacterial colony within 24 hours if the food ingested is, for example, insufficiently boiled milk-based or non-milk-based porridge whose living matter has retained its vitality.

It is therefore sufficient, to definitively and completely eliminate diphtheria, to subject foods containing cereal flours to a treatment that prevents their living matter from developing mycelium and bacteria.

6. False membranes can be developed by conidial forms of barley, wheat, and rye other than Cladosporium herbarum and, more rarely, by Cladosporium of other plants or other conidial forms that come from them.

I determined the formation of false membranes on the rabbit's ear by inoculating spores of *Penicilium* and *Citromyces* obtained by culturing crushed grains or conidia of barley smut.

A) Plate 69 shows the mycelial and bacterial elements of a false membrane developed on the bare dermis of a rabbit's ear by inoculating *Penicillium* spores obtained by culturing on agar from the grinding product of a sanitized rye grain.

Figures 1 to 2 of Plate 69 show various regions of the dissociated false membrane where both the conidia of the mycelium constituting the false membrane and the bacterial elements can be distinguished; the conidia are round or oval; rare long bacilli are seen, others medium and short, as well as cocci. A few very large bacilli with rounded ends are also seen; some bacilli are arched, and many have one end larger than the other; all take Gram staining. Two patches of such bacilli are seen in Figures 2 and 3.

B) By culturing barley smut on agar, I obtained two distinct cultures of *Citromyces*; one greenish-blue, the other white. I think they differ only in the beginning of the evolution of the first towards the *Penicilium* type. I inoculated the blue culture on the ear of one rabbit, the white on the ear of three others, one of which died 72 hours later. Plate 63 contains photographs of smears of these four false membranes.

The first (blue-green *Citromyces*, figs. 1, 6, 7, one-month-old false membrane) shows bacilli of the usual form in diphtheria false membranes and which take on Gram stains.

The second (white *Citromyces,* fig. 2, 7-day-old false membrane) shows identical elements, taking on Gram stains.

The third, that of the rabbit that died after 72 hours (white *Citromyces*) was already beginning to form, and shows (fig. 3, 4, 5) in figure 3 oval conidia with their conidiophores; then, in the other two figures, the short bacillus form. All these bacillary forms take on Gram.

The fourth 7-day false membrane (figs. 8, 9, 10, 11), developed by a broth culture of white *Cytomyces*, shows bacterial elements with varied and characteristic forms found

in diphtheria false membranes; these elements take on Gram. Thus, the false membranes of these two forms of *Citromyces* would have been recognized as diphtheria.

C) I obtained an *Aspergillus* form by cultivating the crushed product of a sanitized barley grain on agar. The spores of this *Aspergillus*, transferred to the broth, produced a veil whose elements (Fig. 13, Pl. 65, photographed on the 6th day) are typical diphtheria bacilli, strictly identical to those in Figure 7 (same Pl. 65), which is a photograph of a 2-day culture, obtained by subculturing in broth a bacterial culture on agar from spores of *Cladosporium* from barley. These bacillary forms, showing their polar bodies, are also identical to those that are born on the mycelium developed in broth by the conidia of *Cladosporium* from barley (Pl. 64, Fig. 8).

Thus, whether it is the living matter itself in nature or its various conidial forms, *Penicilium, Citromyces, Aspergillus*, or *Cladosporium*, the bacterial product of their culture, either in vitro or on the rabbit's ear, is always a characteristic form of the diphtheria bacillus.

This return of various conidial forms of the same living matter to a single form is understood by considering that a pure bacterial culture gives rise, through various changes in the environment, to multiple conidial forms, up to 5 or 6, and even more. A demonstration of this fact will be found in the first volume of this work, for pure cultures of most known viruses.

This phenomenon therefore appears reversible, with each conidial form being able to return to the original single bacterial type through one or more appropriate and successive changes in the culture medium.

Let us also remember that, if the original bacterial form gives rise to a mycelial form with very large branched filaments, this can in turn generate several bacillary forms of different lengths and sizes and several forms of isolated or chained cocci. Demonstrations of this fact have been given elsewhere.

7. The various original sources of the diphtheria virus. Forms in which it causes contamination.

In the first volume of this work, I set out the facts (p. 501) that motivated my first determination of the nature of the diphtheria virus and its original source. I recall them here:

I had noticed that the *Penicilium* forms of the living matter of barley and wheat bear a striking resemblance to the *Penicilium* forms obtained from diphtheria cultures; those of rye also come very close.

At that time, I had also based my determination on a comparative study of the bacterial forms obtained by culturing the living matter of the three species of cereals.

From this study, I concluded that it was the bacterial elements from the barley grain that showed the most perfect identity with the diphtheria bacilli; the bacilli from the wheat grain shared characteristics with them. As for the cultures obtained with rye, I observed that they contained a short coccobacillary form with two polar globules, which is specific to the species and which I had not observed in the cultures of barley and wheat, nor in the diphtheria false membranes of barley. This fact, as well as the slight differences between the *Penicilium* form of this cereal and the form obtained from the diphtheria bacillus, had determined me to eliminate rye from the current research.

The fact that I had succeeded in transforming the diphtheria bacillus into a mold of the identical ochre color that I obtained by culturing the flamed and crushed barley grain led me to conclude that barley is the diphtheria virus.

I thought I had found confirmation of the accuracy of my determination by comparing the respective virulences of broth cultures of wheat and barley. The injection of one cubic centimeter of barley culture kills the rabbit in 1 to 3 days. The same dose of wheat culture kills it in 6 to 12 days, a fact that explains the difference in virulence observed in cultures from different false membranes. Furthermore, the lesions caused in the guinea pig by the barley culture are much closer to those caused by cultures of highly virulent diphtheria false membranes.

In reality, these facts did not prove that the living matter of barley is the only diphtheria virus; they only proved that it is more virulent than that of wheat.

The facts presented in this chapter provide such precise and formal evidence that it is no longer possible to cast the slightest doubt on the nature of the diphtheria virus and its original sources. They also show that rye is also capable of forming a false diphtheria membrane.

It can also be stated with certainty that it is other plant species that develop pseudo-diphtheria false membrane tonsillitis.

Other species can infect the pharynx, without the formation of a false membrane, and cause tonsillitis, which is the beginning of a general infection, or chronic and serious illnesses, such as, for example, common tonsillitis, which is known to be followed by fatal nephritis in the long term. In the latter case, it is very likely that the nephritis, like that of scarlet fever, results from the development of the mycelium of the infecting agent in the parenchyma of the kidney.

8. The different conidial forms of the living matter of the same food can cause different diseases. It is the conidial form that creates the character of each disease. Two conidial forms of the same food, developed together, can cause two simultaneous diseases.

We will see later that scarlet fever is probably caused by the living matter of rye vegetating in the blood in the form of *Penicilium* or *Citromyces*.

My first determination, indicating the carrot as the original source of scarlet fever, was not exact, because identification by comparing *Penicilium* forms is extremely difficult. I subsequently had to attach less importance to this comparison, although it did provide me with valuable information, notably that of barley for diphtheria, and that of lemon, orange, and other Aurantiaceae for Malta fever.

The question that now arises is the following:

The plant, the original source of a virus, being determined, is it its living matter that directly causes the disease or is it one of the hyphomycete forms to which it gives rise while vegetating?

The study just carried out for diphtheria shows that it is the *Cladosporium, Citromyces, Aspergillus*, and probably *Penicilium* forms of the living matter of cereals that are the initial virus.

An additional clarification is necessary, however, regarding this latter form because the mycelial elements contained in the blood of patients with measles and scarlet fever appear to be conidial forms of *Penicilium* or *Citromyces.*

Other facts well known to clinicians, which I had not taken into account in my initial research on the original source of the virus of these diseases, teach us that measles or scarlet fever can develop in the same patient from the onset of diphtheria or during its development; it is also known that measles and scarlet fever can develop simultaneously from the onset or during the development of each of them.

It follows from these facts:

1. That it is well-defined conidial forms which are the real viruses of the diseases and the form in which the living matter of the food virus vegetates in the organism;

2. That the simultaneous development of diphtheria and measles or of diphtheria and scarlet fever indicates that contamination of the two diseases occurs through the same food;

3. That the simultaneous development of measles and scarlet fever confirms the previous conclusion;

4. That the flours of food grains (wheat, barley, rye) are, in all likelihood, the viruses of measles and scarlet fever.

My first identification, which indicated lettuce as the original source of measles, was therefore inaccurate.

In the case of the spontaneous development of two diseases, the exact composition of the food must be known; cereal flours sold for children's food are often mixtures of several species. In this case, the two diseases may come from two different species. If the food contains only one species, two different conidial forms of that species are the viruses.

These facts are explained by the following two principles, which have been the subject of multiple demonstrations in the first volume of this work:

1. The living matter of an organized being, aseptically extracted from the organism and cultivated in broth or on various solid media, gives rise either to a bacterial culture or to multiple hyphomycetes of varied conidial forms: *Cladosporium, Sporothricum, Fusarium, Spicaria, Citromyces, Penicilium, Aspergillus, Mucor*, etc.

2. Most conidial forms are not stable and evolve quite easily into another form, generally of a higher rank.

For example, the spores of barley and wheat smut easily give rise to *Cladosporium herbarum*. The urediniospores and teliospores of black and yellow rusts of wheat, barley, and rye also easily give rise to several conidial forms.

This principle shows that the living matter of the grain of cereals remaining adherent, even in small quantities, to the orifice of the tonsillar crypts or at some other point in the nasal cavities or the pharynx can develop there into one or more conidial forms of hyphomycetes which will be the origin of diphtheria and another disease.

On the other hand, the spores of cryptogamic diseases which contaminate flour, *Cladosporium*, smut, rusts, wheat bunt, etc., can remain adherent to the pharynx and directly cause the formation of one or two pathogenic conidial forms. There can thus be two simultaneous infections.

During the course of the disease, the virulent conidial form may transform into another, causing the development of a second disease, measles or scarlet fever. It is also possible that the same food that caused the first infection continues to be given to the patient and causes a second, different one 8 or 15 days after the first.

Cryptogamic diseases of cereals can be observed with very little development in dry years. Smut is almost always found on ears of wheat, barley, or oats, regardless of the year.

But it is in very hot, rainy, and stormy years that cryptogamic diseases develop very strongly on the grains, glumes, stems, and leaves. The lodging that occurs during storms exposes the grains to becoming covered with pathogenic conidial forms upon contact with the soil. In these years, we find on the ears and grain of cereals, mainly: *Cladosporium herbarum*, smut, and bunt of wheat, and on the leaves and stems: *Cladosporium herbarum*, black, yellow, and brown rusts, *Erysiphe graminis*, etc.

It is impossible to eliminate the diseased parts before threshing, and during threshing, the impacts of the threshing machine on the ears, leaves, and stems cause the spores of these diseases to fall with the grains, and they are inevitably mixed in large quantities with them.

By the autumn of the following year, all these spores will be spread in the flours food and delivered for consumption. Depending on the region, the flours will be more or less contaminated; those from cereals in very humid regions will be more so than those from dry regions; contamination will be greater for varieties grown in a climate and soil that are not suitable for them.

These contaminated flours can be distributed in limited regions, in a single town, in a single part of a town, or in an entire department, and they will cause an outbreak of numerous cases of diphtheria in these regions where they will take on an epidemic form, but only in appearance because the infection will occur through ingestion of the flours and not through contagion of individuals.

These flours can also spread measles, scarlet fever, and especially infantile polio, the virus of this disease evidently existing in two of the most common foods given to young children from 6 months to 3 years, then up to 5 or 6 years in decreasing proportions, food consisting of flour from cereals contaminated by cryptogamic diseases, and milk soiled by stable manure.

Thus is determined the cause and nature of the crucial influence of meteorological conditions on the development of epidemic-like diseases.

Thus also appears the capital importance of determining the nature and the original source of viruses and the real form in which they initially act. It is the knowledge of this original source that allowed me:

1. To determine the vegetable and food nature of the principal viruses;

2. To determine that it is through food ingestion that infection by heterogeneous viruses occurs and not through contagion between individuals, which is only accidental and very rare, as proven by the observations made by **Marinesco** during the 1927 poliomyelitis epidemic in Romania.

3. To establish that it is a specific conidial form of the living matter of a food plant that causes a disease and that this same plant can also cause, through ingestion, one or more other different diseases caused by the various conidial forms of hyphomycetes to which its living matter can give rise.

These notions of the alimentary nature of heterogeneous disease viruses, the determination of their original source and the means of achieving it, and the explanation of multiple and different infections by the same food source are therefore of considerable importance for Etiology, Pathogenesis, and the fight against infectious diseases.

This importance is so considerable that it can be predicted that, in the near future, and if we want it, all eruptive fevers, diphtheria, poliomyelitis, and other heterogeneous infectious diseases such as typhus and the plague will have disappeared from the list of human diseases, without vaccines and only through precise food hygiene.

9. Diphtheria toxoid can only protect against one of the diphtheria viruses, the one that was used to manufacture it. The multiple diphtheria viruses are specifically different, either by their origin (wheat, barley, rye), or by their initial conidial form, and therefore, toxoid prepared with one of them cannot protect against the others.

Behring, Roux and their collaborators could not have suspected, due to their ignorance of the multiplicity of diphtheria viruses, their initial form and their original source, that their experiments on animals were not applicable to humans; the fact of experimenting with the toxin of a single virus that was used to immunize one animal, then injecting a second with this same toxin mixed with the antitoxin, proved the preservative action of the latter against this single virus but not against the others that are specifically different and against which it can only be inactive.

This is the fundamental cause of the ineffectiveness of toxoid and anti-diphtheria serum, proven long ago by practice.

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The conclusions of **Behring** and **Kitasato**, as well as those of **Roux**, were therefore an extremely serious error which, unfortunately, led to the implementation of anti-diphtheria serotherapy; this, itself, constituted a second error which had equally serious consequences since it led to the contamination of all injected animals by the organic colibacillus of the horse.

Let us recall that the notion of the toxin as a soluble poison is false; it is the micrococcal granules of the virus which constitute its active element and not a soluble poison; formal proof of this is given by the fact that, exposed to atmospheric oxygen after filtration, the granules which constitute it evolve and reproduce the bacillary form characteristic of the virus.

Injecting the toxoid is therefore injecting the formed elements of the virus; these, by multiplying, confer diphtheria on the inoculated person.

To vaccinate reliably, it would therefore be necessary to inject a polyvalent toxoid containing the six or nine viruses of two or three conidial forms of wheat, barley, and rye; such an operation is not admissible because it would be criminal to introduce into the human body such a number of dangerous viruses, all of which will confer the chronic phase of specifically different diphtherias.

It therefore becomes imperatively necessary for those who claim to immunize against diphtheria to demonstrate:

1. That diphtheria toxoid immunizes against infection by *Cladosporium* of wheat, barley, and rye;

2. That it also immunizes against diphtheria of other conidial forms of cereals;

3. That an anatoxin, made only with the culture of *Cladosporium* diphtheria of barley, immunizes against diphtheria of wheat and rye, and against that of other conidial forms of cereals;

4. That, reciprocally, an anatoxin prepared with a culture of *Cladosporium* diphtheria of wheat or rye immunizes against C*ladosporium* diphtheria of barley.

Such demonstrations must include the fact that, in the immunized rabbits, the false diphtheria membrane can no longer develop by infection of the bare dermis of the inner surface of the ear.

Until these demonstrations have been made, there will be no proof of a real preventive action of the anatoxin.

Thus, it is under these conditions of ignorance of the real nature and multiplicity of diphtheria viruses, of ignorance of their specific differences, of ignorance of the real possibilities of the action of toxoid, that inoculation with it is practiced and has even come to be made compulsory for schoolchildren and for the army.

The facts just presented demonstrate that the supposed immunizing action of toxoid is based only on gratuitous assertions and that it has yet to be demonstrated.

By categorically affirming the absolute efficacy and harmlessness of diphtheria toxoid, the public has been knowingly deceived. This was not an error, but a self-serving lie.

To prove this, let us examine whether the impossibility indicated above of obtaining immunization with toxoid due to the multiplicity of diphtheria viruses is contradicted or verified by practical results.

All doctors now know that those vaccinated with diphtheria toxoid contract diphtheria in roughly the same proportion as those who are not vaccinated, and that the diphtheria they contract is as serious as that of those who are not vaccinated (**Friedberger**).

P. Ruttgen and **Isi Fischer** observed 13 cases of diphtheria out of 48 vaccinated people, or 25% (Bull. Med., December 3, 1932, p. 839). Note that the proportion of diphtheria among the unvaccinated is significantly lower.

A doctor from Besançon (Doubs), **Dr. Tronchon**, outraged by the false claims contained in a poster concerning diphtheria vaccination posted in a school in Besançon Saint-Claude, had it removed. When the Prefect asked him for an explanation, he replied that he had had it removed because it contained dangerous errors since it stated that vaccination permanently protected against diphtheria and that a vaccinated child would never again need serum. However, he himself had observed 13 cases of diphtheria in vaccinated children.

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At its meeting of February 12, 1932, the *Union Syndicale des Médecins du Doubs* decided to publish in the regional press a note refuting the errors propagated by the posters posted in schools and that, at the same time, a letter would be written to the Prefect and the Mayor to request the removal of the incriminating posters.

Doctor **Ch. Bellet** wrote **(1)**:

Here are four observations that tend to prove that toxoid is not the panacea advocated by the Pasteur Institute:

I. *Child Ch., rue Audran, received three toxoid injections in 1931. In 1933, she developed severe diphtheria, requiring hospitalization. The three other unvaccinated children, who remained in contact in a miserable, poorly ventilated room, having received no serum injections, did not contract diphtheria.*

II. *Child L., rue A.-Tourres, vaccinated with toxoid, contracted diphtheria the following year. The older brother and sister, younger, very feeble-minded, not isolated, were not affected.*

III. *Child V., avenue Emile-Dupont, 4 years old, vaccinated with toxoid in 1935, has just contracted diphtheria, requiring admission to the General Hospice. The three other children, including two younger ones, who were unvaccinated, were unharmed, without any precautions having been taken.*

***(1)** I took the information I give here and above from the book by Doctor Paul Chavanon entitled "On peut tuer ton enfant" (You Can Kill Your Child). Editions Médicis.*

IV. *In 1934, during a public vaccination, I received a note from a mother refusing to have her child vaccinated because, she told me, she had just lost two children to diphtheria, the disease against which they had been vaccinated.*

These observations demonstrate, in addition to the ineffectiveness of vaccination, the minimal danger of contact with the sick for contamination, which occurs almost exclusively through ingestion of the virus.

These four observations are presented by a doctor, and there are more than thirty thousand of them in France. This shows how common diphtheria must be among vaccinated people. I will limit myself to these few examples taken from the book by Doctor **Paul Chavanon.**

The statistics published in Greece and France prove that vaccination with toxoid has not reduced the number of diphtheria cases; on the contrary, this number has increased sharply; in Greece, where preventive toxoid inoculations have been practiced since Oct. 1926, the number of diphtheria cases increased from 750 in 1929 to 1,840 in 1934.

In France, the number of diphtheria cases rose from 11,033 in 1923, when toxoid was introduced, to 14,259 in 1927, then 18,898 in 1928, and then to almost 21,000 in 1933.

In Germany, where vaccination is carried out with a mixture of toxin and anti-diphtheria serum, the proportion of diphtheria cases rose from 30,300 in 1926, to 77,300 in 1933, then to 119,100 in 1934 and 146,700 in 1937; in 1935, 82.3% affected children aged 2 to 14.

Doctor **Léon Renard**, of the Léopold Bellan Hospital, wrote in June 1938 in the *Revue Française* that, since schoolchildren are almost all vaccinated, it cannot be claimed that, if vaccination were even more widespread, cases of diphtheria would be fewer, when the opposite is true. Doctor **Renard** then adds:

On the contrary, it often happens that some vaccinated people contract diphtheria during vaccination, and this diphtheria is very serious and seroresistant.

*... Moreover, vaccinated children often have much more serious diphtheria than if they had not been vaccinated **(2)**.*

Here is the danger of diphtheria vaccination; if the French accept the risks without protest, they will soon be given a nice little law that will make BCG vaccination compulsory, which does not vaccinate anything at all, which, on the contrary, is illusory; we have lost count of the cases of death from meningitis, etc., due to this vaccination.

We must not believe that vaccinations, even smallpox vaccination, are free from danger.

***(2)** By vaccinating children, they are safely given one of the different diphtheria viruses and the chronic phase of this diphtheria. Perhaps, through this, they are sensitized to other diphtheria viruses.*

Haven't numerous cases of encephalitis, neuritis, blindness, and even death been reported following the latter? So much so that in Holland, smallpox vaccination was banned a few years ago, and our intelligent English neighbors do not allow this vaccination either. Consequently, no vaccinations.

Due to untimely vaccinations, the morbidity of each individual is increased, their resistance to future diseases is reduced for a problematic condition that they might never contract.

These conclusions are perfectly accurate, as we will see later.

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In summary: **The toxoid inoculation thus proves to be practically ineffective, illusory, and useless, because it is made up of a single virus and because, since diphtheria viruses are multiple (six and certainly more) and specifically different, it cannot act against the others.**

It is therefore impossible for the toxoid to have an immunizing effect; this is scientifically and surely demonstrated by the new concepts set out above.

But, despite the peremptory proof of ineffectiveness provided by both practice and statistics, the effectiveness of anti-diphtheria toxoid continues to be affirmed through intense propaganda and commercial advertising that is not likely to inspire esteem and consideration.

I have before my eyes, holding a full page of a fashion magazine, a commercial advertisement bearing the headline: **Croup will soon be nothing more than a dreadful memory.**

Then, further down, these mendacious lines:

We are assured that this decision (the law making vaccination compulsory for children) was taken wisely, after a great deal of experience, and that thanks to this measure, diphtheria will disappear from our country just as smallpox has disappeared since we have been subjected to smallpox vaccination, whereas this disease previously claimed tens of thousands of victims every year in France.

Will croup soon be nothing more than a dreadful memory, a terrifying image forever vanished? This is the fervent wish of all mothers.

Is it possible to deceive the public in this way when it has become clear that vaccinated people contract diphtheria at least as easily as unvaccinated people, and when statistics show that, despite vaccination, the number of diphtheria cases doubled from 1923 to 1933 in France and, in Germany, quintupled from 1926 to 1937 (30,000 to 146,000).

The considerable number of reports in medical journals relating to accidents that occurred either during vaccination or immediately afterward is incontestable proof of the harmfulness of toxoid.

The accidents that occurred when toxoid alone was inoculated are of two kinds:

1. A rise in temperature of up to 40° immediately after the first injection, accompanied by various discomforts, headaches, body aches, vomiting, severe pain, or hives, and meningeal symptoms. Generally, after a strong reaction, doctors refrain from the other two injections.

The reaction can be even more violent and cause death after two or three weeks of various symptoms, vomiting, coma, etc. (case of the young Coignet).
(See the book by Doctor P**. Chavanon**. Preface).

2. Similar symptoms following the 2nd and 3rd injections. Appearance of diphtheria, even after the first injection;

3. Symptoms occurring 2 to 3 weeks after the third injection. These are the symptoms of the chronic phase of diphtheria. Violent nephritis (28 g of albumin) in the young Verrier, greatly improved by eight years of treatment. Then aggravated by trauma, ends 1 month 12 later in death.

4. In all cases, whether there is a febrile reaction or not, the inoculation of the toxoid, a live virus, infects the vaccinated person and gives them the chronic phase of a form of diphtheria, a chronic phase that lasts as long as the virus remains alive in the body and can cause various disorders throughout this duration. The febrile reaction that follows the first and often the other injections of the virus is the acute phase of the diphtheria infection.

The harmfulness of diphtheria toxoid is therefore very real and indisputable. It confers the chronic phase of diphtheria and causes serious disorders which, while not frequent, are nonetheless real. The known cases of death are quite numerous.

Those who doubt should read the abundant bibliography of these accidents and also read the book by Doctor **Paul Chavanon**. They will be convinced.

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Moreover, it appears that the vast majority of the medical profession is now convinced of the total lack of effectiveness of diphtheria toxoid and the dangers of its inoculation. Therefore, very few doctors today agree to vaccinate their own children.

In summary: Vaccination with toxoid is ineffective, illusory, and useless. Moreover, it is harmful; it can be dangerous and cause fatal accidents. In any case, it inoculates the vaccinated person with diphtheria. Since it serves no purpose, it must be avoided.

Under these conditions, doctors must refuse to perform this vaccination and they have a duty to inform those concerned about the dangers and the uselessness of this practice; they have a duty to inform them that vaccination will not prevent vaccinated children from contracting diphtheria as well as unvaccinated ones.

They will fulfill a much more important social role by informing their clients that there is a sure way to avoid diphtheria:

First, they must inform them that diphtheria is contracted exclusively through cow's milk contaminated by stable manure, and through cereal flour, both of which contain the virus, and that, to avoid being contaminated by it, it is sufficient to kill it by heat, that is, to boil for a long time the milk and cereal flour porridge given to children, or other foods containing these flours.

It is even safer to subject these foods for 20 minutes to a temperature of 120° in a small commercial pressure cooker.

10. Anti-diphtheria serum. It will be demonstrated in the chapters: *"Mechanism of Blood Coagulation"* and "*Colibacillus Function of Vertebrates*" that the organic disorders caused by an injection of serum are due to the presence, in it, of a considerable mass of elements of the organic colibacillus of the animal that provided the serum. The injection of horse serum, for example, is therefore equivalent to the inoculation of the organic colibacillus of this animal.

The anti-diphtheria serum prepared by injecting the horse with successive doses of diphtheria toxin is subject to exactly the same criticisms that have just been leveled against toxoid. I will therefore not repeat them here. I will simply point out that it is obtained with the culture of a single diphtheria virus, whereas there are six and more.

This serum apparently had an extremely beneficial effect at first. But it was not long before its therapeutic effect declined. The *"Journal des Praticiens"* of November 6, 1926, presented the question of serums as follows:

Serum medication is beginning to trouble the conscience of the medical profession. Serums are less active. Where once 20 cubic centimeters of anti-diphtheria serum were needed, today 60 are required. Other serums, such as the anti-meningococcal serum, are losing their effectiveness. The anti-meningococcal serum B, as Professor ***Teissier*** *has demonstrated, remains virtually ineffective. One might even wonder if it sometimes makes matters worse.*

On the other hand, the disadvantage of lesser effectiveness is compounded by another inferiority. Serums are more toxic. They heal more poorly and poison more. In the past, serum accidents were, if not rare, at least benign. Today, catastrophes have multiplied. Heterogeneous albumins, it is said, are the cause of accidents. Let us then eliminate heterogeneous albumins. Once this has been eliminated, will the serum still enjoy its curative properties?

The author of the article then describes the accidents and disorders resulting from the injection of serums.

It should be noted that the disease in which serum causes the most accidents is ***asthma****. In the past,* ***asthma*** *was not always cured, but it only very rarely killed. When neither the heart nor the kidneys were affected, patients lived for many years. Now, Lanson cites five deaths from* ***asthma****. The antidiphtheria serum, or normal horse serum, had done all the damage. However, the death toll is far from exact, and the real number always exceeds the published figures.*

A first injection can cause death. Vedel and Puech injected horse serum into a chronically feverish person. Eight days later, fever increased, generalized nettle rash. Four days later, enormous edema of the face and eyelids. The next day, the patient died. At autopsy, a certain degree of liver failure was found, and that was it.

Deroide, from Calais, injected antitetanus serum into a 6-year-old girl. She died a few minutes later. Lesne and Barreau dealt with a 4-year-old child with benign diphtheria angina. Died a few minutes after a first injection of 50 cc of antidiphtheria serum. Voisin and Terrien had to deplore similar disasters.

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The cause of death is infinitely obscure. Anaphylaxis does not explain everything, and there are facts that defy interpretation. Liver failure, which some people accuse of, is not always detected. It is understood that death is rare (about 1 in 75,000).

Still, doctors have been warned. Especially since the number of accidents has increased significantly. Urticarial and arthralgia eruptions, initially reported, now represent only one of the elements of the disorders that can appear. Serum adenitis (Mouriquand and Dechaume), serum orchitis (Blechmann and Stiassnie), amyotrophic paralysis following polyneuritis (Pollet), pseudotabes (Babon-neix), and serum pleurisy (Sabrazes) have been observed. Above all, polyneuritis and pseudotabes appear as serious complications. They can persist for many months. Such consequences encourage the practitioner to be cautious. He will not inject serums haphazardly.

The explanation for these accidents is provided by the presence of organic colibacillus of the horse in the serums. It is this, and not the heterogeneous albumins introduced

into the blood, which creates the state of anaphylaxis, which causes anaphylactic shock and is the disease of organic colibacillus of the horse.

Such observations have not only been made since 1920-26. Indeed, as early as 1910, in a remarkable book entitled Errors and Deceptions of Modern Medical Science (Paris, P. Daillère, 1910), Doctor **Bourget,** professor of clinical medicine at the University of Lausanne and an eminent scholar, explained (pp. 58 to 68) that, a convinced supporter of the specific effect of anti-diphtheria serum since 1895, he gradually refrained from using it thereafter because he had observed that it caused certain disorders (exanthema, rash, albuminuria with casts, generalized edema, syncope, arthralgia, furunculosis, etc.); he now only injected it into seriously ill patients.

On page 60, he writes: "*Today (1910), most clinicians affirm that antidiphtheria serum has no effect on cases of croup*."

Through his clinical observations on 589 cases of diphtheria, **Bourget** demonstrated, as early as 1910, that antidiphtheria serum has no effect on the progression of diphtheria. Of these 589 cases, there were 15 deaths out of 189 injected cases, or 8.1%, and only 2 deaths out of 404 uninjected cases.

Bourget also noted that the detachment of the false membranes, which was claimed to result from the specific action of the serum, was observed just as well in uninjected subjects.

Today, this series of observations receives clear and reliable explanations that prove their accuracy. The lack of therapeutic effect of the anti-diphtheria serum results from the fact that, since diphtheria viruses are multiple and number six or even more, the serum prepared with only one of them cannot act on the others.

The following case is a very demonstrative example: the Chief Physician of the Diphtheria Department at Trousseau Hospital presented me with a patient, about fifty years old, who was treated without effect with about 400 cc of anti-diphtheria serum and who, three weeks later, was struck down with a second case of diphtheria, the false membrane of which I studied. In this case, it was certainly a question of two different diphtheria viruses, neither of which corresponded to the virus used to make the serum.

11. Avoiding the disease is better than immunizing against it. The fight against diphtheria must consist of avoiding the ingestion of the live virus and not of inoculating this virus, even though it is attenuated.

It is better to avoid the disease than to immunize against it because the introduction of the live virus into the body, although attenuated, necessarily confers the mycelial and chronic phase, a phase which lasts as long as immunity and whose symptomatology we

are completely ignorant of with regard to vaccinations against smallpox, typhoid fever, etc. Knowledge of human and animal diseases is still very little advanced.

The initial cause of many varieties of nephritis (notably Bright's disease), arteriosclerosis, many nervous diseases, and many skin diseases is still unknown.

However, the existence of scarlet nephritis and nephritis following other infections demon strates that this chronic phase of any heterogeneous virus disease, which medicine is still ignorant of, is very real, is not imaginary, and that the lesions caused in the organs by the virus in the mycelial state can be extremely serious.

Let's take smallpox as an example. Its acute, bacterial phase is very short, because the mycelial phase begins as soon as the rash appears. We will see, when studying smallpox and vaccinia, that the vaccinal (or variola) pustule is formed by the mycelium of the infecting agent and that this mycelium completely destroys the dermis. The proof of the virus's transition from the bacterial state to the mycelial state is therefore clear and certain here.

For smallpox and vaccinia, immunity therefore coincides with the mycelial state of the virus; we are currently completely ignorant of where their mycelium is located. Doesn't this determine serious disorders, for example, a form of arteriosclerosis or a form of Bright's disease? Cases are not very rare of children whose vaccinia was severe and who remained in a precarious state of health for a long time.

The state of anaphylaxis created by the inoculation of horse colibacilli into humans or animals lasts twenty, thirty years, or more, which means that the latter remains alive and present in the blood and tissues throughout this time. The numerous pathological manifestations reported in the Journal des Praticiens article cited above, some of which "can persist for many months," are formal proof of the chronic mycelial phase that follows the injection of organic colibacilli contained in a serum, and even more so of other, much more virulent, heterogeneous viruses. This chronic mycelial phase of heterogeneous virus diseases is therefore well established and confirmed by clinical observation.

In view of these facts, is it desirable and even admissible that, for a very low risk of contracting diphtheria, we confer the chronic phase of diphtheria, this time with a 100% risk, by inoculating all children with anatoxin, a live virus?

If we compare the number of children in schools in the city of Paris with the number of cases of diphtheria observed there in a year, we will realize how low the risk is.

If, on the other hand, we consider that it is mainly children from 1 to 6 years of age who contract diphtheria, and this is because of their diet, which includes the food virus, and if these cases of diphtheria are counted in the total number for the year, we see that, if we deduct from the latter the cases of diphtheria from 1 to 6 years of age, we would see that the number of cases occurring among schoolchildren is tiny compared to the total number of these. It is therefore to avoid a tiny risk that the vaccination of schoolchildren with toxoid has been made compulsory. Let us note, moreover, that the proof of the lack of immunizing action of this vaccine has now been practically established and that the multiplicity of specifically different diphtheria viruses explains this lack of action. This vaccination is all the more unacceptable since, since the original source of the virus is known, it is sufficient to prevent diphtheria:

1. Boil cereal porridges intended for children for a minimum of 20 minutes;

2. Boil milk intended for food for at least 20 minutes.

An even safer method consists of maintaining the milk and its porridges at a temperature of 110° for 20 minutes in small commercially available pressure cookers. To eliminate the possibility of contamination by ingestion of the diphtheria virus, it is necessary to destroy the vitality of the living matter in cereal flours and the spores of the cryptogamic diseases they contain. All of these facts therefore demonstrate:

1. That protection against diphtheria is assured, in an absolute manner, by the destruction of the virus by heat and that it is not at all necessary to immunize against this disease by inoculation with a live virus which, moreover, is ineffective and which has the disadvantage of conferring the chronic phase of the disease;

2. That the risks of contamination run by schoolchildren are minimal and that the disease is not contracted by contagion or contact with sick people, but by ingestion of the virus with food containing it;

3. That consequently, there is an urgent need to abolish the obligation of vaccination with toxoid in humans and especially in schoolchildren.

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Summary of the new concepts presented in this chapter.

These new concepts are summarized in the following conclusions:

1. The diphtheria false membrane is formed by the felting of the mycelial filaments of a hyphomycete giving rise to conidia and bacillary elements;

2. This hyphomycete is one of the conidial forms *Cladosporium herbarum*, *Citromyces* and *Aspergillus*, of the living matter of wheat, barley and rye;

3. The diphtheria bacillus of the false membranes is not the initial infecting virus; it is a production of the mycelium of the false membrane, a mycelium which precedes the formation of diphtheria bacilli and other bacterial formations;

4. The initial diphtheria virus is the hyphomycete that forms the false membrane, or the living matter of the grain of grasses itself, giving rise to this hyphomycete prior to the false membrane;

5. The culture in broth or on agar of the conidia of *Cladosporium herbarum* of barley, wheat, and rye gives rise to the diphtheria bacillus with all its characteristics;

6. The conidia of *Cladosporium herbarum* of wheat, barley, and rye, deposited on the bare dermis of the inner surface of a rabbit's ear, form a false diphtheria membrane there. The same is true for the conidia of other conidial forms, *Citromyces, Penicilium*, etc.;

7. Conversely, a broth culture of identified diphtheria bacillus can transform into a film of *Cladosporium herbarum*; it can also undergo this transformation on solid culture media if it is transported there;

8. The living matter of barley, wheat, and rye grains, externally sanitized and pulverized, cultivated in broth, develops in 24 or 48 hours a veil formed of typical diphtheria bacilli;

9. The living matter of barley, wheat, and rye grains, externally sanitized and pulverized, deposited on the bare dermis of a rabbit's ear, develops a typical false diphtheria membrane;

10. The mycelium of the false diphtheria membrane, formed by *Cladosporium herbarum*, gives rise to mycelial filaments whose diameter varies from 0.5 to 6 or 8 microns and which can all segment into bacterial elements which are diphtheria bacilli in the same way as the finest bacilli (**Klebs-Loefler** bacillus).

The finest filaments frequently segment into chains of short cocobacilli or cocci (streptococcus), or into cocci that become isolated or remain attached in pairs; most often, filaments about 0.5 to 1 micron in diameter first segment into bacilli in which segmentation continues until they reach the cocci stage; the final segmentation leaves them in the state of diplococci or separate cocci.

All these bacterial forms are an integral part of *Cladosporium herbarum* and belong to a single species. They are all, in the same way, whatever their size and shape, diphtheria bacteria.

They are therefore not an association of diverse microbial species, as has been concluded. The notion of so-called microbial associations, a consequence of the false dogma of bacterial monomorphism, is therefore one of the biggest errors in bacteriology;

11. Diphtheria viruses are multiple and all specifically different from one another. They consist in particular of the conidial forms *Cladosporium, Citromyces*, *Penicilium* and *Aspergillus* of the cereals barley, wheat, and rye;

12. The spores of autogenous cryptogamic diseases of cereals inaccurately reputed to be parasitic, smut, wheat bunt, rusts, *Cladosporium*, etc., are more resistant to heat than the normal living matter of the grain of cereals. They appear to develop with much more ease than normal living matter the virulent hyphomycetes which form the false diphtheria membranes;

13. These same spores also exist on the straw and empty ears of grasses and germinate in animal litter through contact with their urine and excrement. Milk, which is very often soiled by litter manure, can therefore transmit diphtheria by ingestion;

14. Diphtheria is contracted by ingestion of the virus food and not by contact with sick people, as the following three facts clearly prove:

A) The entry point for diphtheria viruses is the pharynx.

B) The original source of diphtheria viruses is cereal flour, wheat, barley, and rye, and, incidentally, milk contaminated by stable manure.

C) Cereal flour porridge is mainly a food for children from 6 months to 4 or 5 years old.

D) It is primarily and overwhelmingly children from 6 months to 5 or 6 years old who contract diphtheria.

PRACTICAL CONCLUSIONS

1. Particles of cereal flour, which remain adherent to the tonsillar crypts, or in any other part of the pharynx, or which may have been projected into the nasal cavities or the larynx by a coughing fit, can, especially if they are soiled by the spores of crustogamic diseases of cereals, and in particular those of *Cladosporium*, develop in 24 hours the mycelium, which will be the origin of a false diphtheria membrane;

2. This development can only take place if the vitality of the living matter of the flour and of the spores which soil it has not been destroyed by heat, a fact which occurs, most often, for flour porridges prepared for children, and which are only subjected to insufficient boiling, especially when they are prepared with milk;

3. These porridges, or the milk absorbed alone, must be boiled for at least 20 minutes; it is even safer to subject them to a temperature of 110° for 20 minutes in a small commercial pressure cooker;

4. Sterilizing cereal flours by heat of sufficient intensity and duration is therefore the sure, infallible way to avoid diphtheria and to make it practically disappear from the list of diseases, without preventive inoculation with any vaccine;

5. Since diphtheria is contracted by the ingestion of the food-virus and not by contagion, the dismissal of schools in which a few cases of diphtheria occur is therefore a useless, ineffective, and unjustified measure;

6. The effect of anti-diphtheria vaccinations and curative serum injections has been manifested so far by the following results, established by medical practice:

For toxoid: Zero protection; vaccinated people contract diphtheria just as easily as unvaccinated people; in addition, the vaccine unnecessarily inoculates them with a dangerous diphtheria virus in the short or long term.

For anti-diphtheria serum: Zero therapeutic effect because scientifically impossible, the serum lacking the essential specific link with the infecting virus. In addition: Safe inoculation of the patient with serum sickness from the organic colibacillus of horses, causing multiple immediate and remote accidents.

* * *

Despite these harmful results, already known in 1938, it was possible, through false and self-serving statements recounted in the report on the explanatory memorandum, to pass a law making this ineffective and dangerous vaccination compulsory.

In this report, it is stated that vaccination would eliminate diphtheria from France. However, from 1923, when it was implemented, until 1933, the number of diphtheria cases in France had increased from 11,000 to 21,000. The public interest was thus sacrificed without the responsible authorities ordering the slightest information or serious investigation.

Currently, and in defiance of the truth, the Pasteur Institute continues to assert the harmlessness and effectiveness of this vaccination, an assertion rigorously contrary to reality.

By acting in this way, it assumes a very serious responsibility, because by wanting to treat the French like guinea pigs, by claiming to forcibly inoculate them, like animals with live, virulent, dangerous, and, moreover, therapeutically and totally ineffective

microbes, under the sole pretext of a false assertion, is the most serious attack that can be made on the freedom of man to dispose of himself as he sees fit, a freedom proclaimed by the Declaration of the Rights of Man of 1789.

The law of June 25, 1938, constitutes a serious attack on the freedom and essential rights of man. It is therefore unconstitutional. Its passage was determined by false assertions. Its effects since 1938 have been harmful and, in many cases, catastrophic for the population. It must therefore be abolished without delay.

Undoubtedly, this law has been a prodigiously fruitful affair for the financial prosperity of the Pasteur Institute; but the health of the French, the progress of hygiene and medical science cannot be subordinated any longer to the financial interests of an institute including a large commercial firm.

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CHAPTER XII

STUDY OF TETANUS AND THE TETANUS BACILUS

STUDY OF TETANUS TOXIN AND ANTITETANUS SERUM

The study of the tetanus bacillus that I made in the first volume of this work (p. 492) sets out the reasons that led me to falsely conclude that the potato is the original source of the tetanus bacillus.

Inoculation of tetanus toxin into the subcutaneous connective tissue of a guinea pig's thigh, followed three or four days later by culture on agar of the pulp of the lung, liver, and spleen, ground with the necessary aseptic precautions, gave rise to a hyphomycete whose very young sporiferous hyphae (fig. 1, 2, 3, pl. 206, 1-; vol.) bear spores pedunculated which have exactly the shape of tetanus bacilli; these spores enlarge and, in the adult state, take on an oval shape with a pointed end on which their pedicle is inserted. This conidial form comes from the tetanus toxin and not from the living matter of the guinea pig, because I have never seen it develop in cultures of its normal organs; it deviates clearly from the type of molds of its living matter. This is an easy point to verify.

I had obtained from the potato a form of *Peronospora* which seemed to me very similar to the previous one and, on the other hand, I had observed the following fact, indicated on page 493 in the first volume of this work: in the juice of a certain rot of the potato, accompanied by butyric fermentation attributed to *Bacillus amylobacter*, I noted the presence of numerous bacilli similar to the tetanus bacillus and bacilli in the shape of a bell clapper.

These are the two facts which had determined me to conclude that the potato is the original source of the tetanus bacillus.

This is a mistake I should not have made because I had observed that the tetanus toxin, contained in a sealed and almost full tube, becomes cloudy at the top and reconstitutes a bacillary culture in 48 hours, if the tip of the tube is broken and air is allowed to enter, taking care to keep this tip capped with a sterile cotton plug. But it is no longer the tetanus bacillus that is developing here, it is the colibacillus.

If I had immediately determined the determinism of this phenomenon, and sufficiently studied the culture obtained, particularly its chemical properties, I would have found that it is a culture of colibacillus, and my conclusion would have been completely different since I had already studied the latter and concluded that, in the animal organism, its origin is autogenous.

Subsequently, the discovery of the colibacillary nature of fibrinferment, the colibacillary function, the nature and origin of connective tissue, the colibacillary nature of staphylococci, streptococci, pneumococcus, euterococcus, septic vibrio, etc., demonstrated that the colibacillary granulation of connective tissue is the element which, by evolving in anaerobic culture, becomes a tetanus bacillus.

* * *

The anaerobic culture of the tetanus bacillus is that of a hyphomycete in a liquid medium.

The hyphomycete form photographed in figures 1, 2, and 3 of plate 206 of the first volume, and obtained by culturing the lung, spleen, and liver of a guinea pig inoculated with tetanus toxin, clearly shows pin-shaped or racket-shaped tetanus bacilli.

This allows us to affirm that the spore-forming tetanus bacillus is not a bacillus, but the stalked spore of a hyphomycete, a fact which explains why it takes Gram-positive bacteria, although the colibacillus from which it originates and to which it returns in an oxygenated environment does not take it.

Verification of this statement is easy to do; it suffices to examine, at high magnification, the deposit of fully developed anaerobic tetanus cultures. The various elements they contain are represented in Figure 2 of Plate 2 of the first volume. These elements show that this is indeed the development of a hyphomycete whose mycelial filaments ramify and bear short branches which, when detached, constitute the so-called sporulated bacilli. The filaments themselves, by segmenting, give rise to the non-sporulated bacillary elements. We are therefore in the presence of a rudimentary hyphomycete culture in a liquid medium.

In addition, in the culture, we find germinative masses which we see at the upper right point of the figure, emitting their germinative filaments; they are clusters of refractile granulations and, in reality, leukocytes. A pure culture of tetanus, transferred to a solid medium, was transformed into several forms of hyphomycetes. One of them, cultivated in broth covered with a layer of vaseline oil, was brought back to the bacterial state; it did not give the characteristic culture of tetanus, but exactly the form of the septic vibrio; it is represented in plate 1 of the first volume. We notice there germinative masses which are very clearly leucocytes. This is one of the first examples which taught me that the germinative masses of bacterial cultures are true leucocytes, and that those of the animal organism are the germinative masses of a bacterial culture which, later, was revealed to be that of the organic colibacillus; this resulted in the knowledge of the bacterial nature living beings (see first volume).

We have to make, on the other hand, observations of the highest interest by examining the constitution of connective tissue; I have shown, in a drawing (fig. 1 pl. 22 first vol.) some of the elements found there.

We see large mycelial branches (four) laterally bearing fine pedunculated spores (five) similar to the tetanus bacillus; some already detached also have the typical pin shape, other spores exist at the end of fine filaments.

We also notice clusters of refractile granules (eight) emitting their germinative filaments; they are exactly similar to those observed in anaerobic tetanus cultures. As a result of these observations, it appears that anaerobic tetanus cultures are anaerobic cultures of connective tissue and the elements they contain, that is, colibacillary cocci and leukocytes.

Study of experimental tetanus

The tetanus bacillus, Nicolaier's bacillus, was discovered by Nicolaier in 1884; he saw that inoculating animals with earth tetanizes them and that the pus from the inoculation site contains pin-shaped bacilli. It was **Kitasato** who, in 1889, first isolated and cultivated the tetanus bacillus.

Knud Faber observed in 1890 that, when a pure culture of tetanus bacillus was filtered through porcelain, the liquid (tetanus toxin) transmitted tetanus just as well as the complete culture. Also in 1890, **Béhring** and **Kitasato** succeeded in immunizing rabbits against tetanus by injecting them with complete cultures of **Nicolaier's** bacillus and then iodine trichloride; they discovered at the same time:

1. That the blood of the vaccinated animal, mixed in vitro with tetanus toxin, neutralizes it;

2. That when injected into animals, the blood of the immunized animal makes them resistant to tetanus;

3. That when injected into the tetanized animal, it cures it.

These first two conclusions were verified as correct. As for the third, **Vaillard** demonstrated that it was false.

The discovery of the qualities of the blood of immunized animals led to the principle of serotherapy, which, unfortunately, came into practice without any control of the virulent properties of animal blood, because the false pasteurian dogma of the asepsis of living organisms was accepted without dispute, notably the asepsis of blood, affirmed by **Pasteur** twenty years previously.

In 1891 **Vaillard** confirmed the antitoxic property of blood but established that the conferred immunity disappears after a few days; in 1892 he then established that the anti-toxin neutralizes the effects of the toxin, but does not destroy it.

In 1893 **VINCENZI** (Rif. Médica 1893) established that there was no antitoxin in the blood of a man who had just recovered from tetanus. However, we already knew, at that time, that a first attack of tetanus does not immunize humans.

Indeed **J. Courmont** and **Doyon** wrote in 1899 in their book Tetanus on page 70: A first attack does not confer immunity, then, on page 81: The serum of a man cured of tetanus does not contain antitoxin (VINCENZI). We also know that people suffering from tetanus cannot vaccinate.

This seemed to contradict the verified fact that inoculating an animal with tetanus toxin confers immunity and produces an antitoxin in the blood. No one noticed this apparent contradiction and no one tried to explain it.

From that moment on, a fatal error was established: that of having admitted that the results of immunization experiments on animals also applied to humans. This error was not only fatal, but catastrophic because it led, since 1893, to the injection of anti-tetanus serum into humans which puts them in a state of anaphylaxis and communicates serum disease to them by inoculation with the organic colibacillus of the horse, which is virulent for humans.

How many millions of men who ran no risk of tetanus have been inoculated with this colibacillosis which exposes them to multiple accidents? How many thousands of men have been killed by the immediate shock of the first injection of serum or by the anaphylactic shock from the second injection? And all this without any benefit because we knew in 1893:

1. That someone suffering from tetanus does not vaccinate;

2. That the blood of a man cured of tetanus does not contain antitoxin.

This established in the most formal way that no protection can be created in humans by injection of toxin or anti-tetanus serum.

This shows how illusory the claim is to currently believe that the injection of tetanus toxoid will immunize against tetanus since even a tetanus attack does not vaccinate and does not form antitoxin in the blood. It will therefore be necessary to explain to the public whether there are new reasons which make it possible to assert the effectiveness of tetanus toxin which has never existed.

How could we have made the mistake of applying the results of experiments carried out on animals to humans? We committed it because we believed that, for man and animals, the tetanus bacillus was heterogeneous, also because we were unaware of the bacterial nature of the animal organism which contradicted the false pasteurian dogma of the asepsis of living beings. It was committed above all because this false dogma forbade thinking that tetanus can be autogenous.

Now, this is the crux of the matter: tetanus is an autogenous disease. The tetanus bacillus has an autogenous origin; it is an anaerobic form of the organic colibacillus, and the latter is a normal constituent of the body of capital importance. This is why a tetanus attack does not vaccinate humans; it is because vaccination cannot exist against a normal constituent of the body, the coli bacillus, of which the tetanus bacillus is only one form.

Tetanus induced experimentally in rabbits and guinea pigs is heterogeneous. This is why it is accompanied by the formation of an antitoxin, as is the case with any heterogeneous virus. But this antitoxin is only active against the toxin that gave rise to it; its activity is specific and cannot be exerted against any other strain of tetanus bacillus or toxin.

When operating with a strain of tetanus bacillus, it is therefore necessary to know its precise origin, that is, the name of the animal from which it was taken during spontaneous tetanus. If an animal is immunized with this strain, the antitoxin in its blood will only be active against it and only against experimental tetanus induced in an animal other than the supplier of the strain. Contrary to what one might expect, the antitoxin will have no effect on the latter because it will be completely fixed by its normal coliform bacillus.

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Thus, even if it were possible to obtain a very powerful antitoxin of **human origin** that would neutralize the **human** tetanus bacillus and its toxin for an animal, this antitoxin would have no preventive or curative effect on humans against a declared tetanus because it would be fixed and monopolized in its entirety by the enormous mass of its organic colibacillus.

The fact remains that the current inoculation of toxoid is an inexcusable fault because the first and exact origin of the toxin is totally ignored and because, consequently, a living bacterial product is inoculated despite total ignorance of its specificity against human tetanus. Thus, in no case can any preventive or curative action be obtained against spontaneous tetanus in humans, neither by a serum nor by inoculation of a culture or toxin.

This explains why a tetanus attack does not vaccinate humans. This is because it is autogenous in humans, as it is in castrated animals.

As can be seen from this short presentation, two new concepts of capital importance have radically modified the aspect of the tetanus question; these are, on the one hand, the bacterial or colibacillary function of animal organisms and, on the other hand, the autogenous origin of tetanus and the tetanus bacillus.

Original source of the tetanus culture and the toxin used for immunizing horses.

At present, bacteriologists teach that the tetanus bacillus, which they consider a distinct, legitimate heterogeneous species, has its normal habitat in the earth, manure, like the septic vibrio, and it is by cultivating plots of land that they are able to obtain cultures considered pure of these two supposedly distinct bacterial species.

It is of capital importance, from the point of view of assessing the properties of antitetanus serum, to know the original source of the tetanus bacillus used to manufacture the immunizing toxin injected into the horse.

Is this origin the earth or manure, or directly the organism of the man suffering from tetanus? If it comes from the earth, its origin is as follows:

The urine of domestic animals, like that of humans, contains countless quantities of colibacillary cocci from the blood. Excrement contains just as many and, in addition, the bacillary form of colibacillus. Stable manure, which is made up of straw, urine, and excrement and is always piled up in large masses, is then spread on the ground, l ike liquid manure. This is why the earth always and everywhere contains the bacterial elements of animals, and these elements are especially those of the colibacilli of horses, cattle, sheep, goats, pigs, and rabbits; they are mostly in the cocci form.

It is because of this constitution of the soil that, when cultivating it, a culture of septic vibrio or tetanus is so easily obtained in an anaerobic environment. But the exact origin of the culture is unknown. We can be sure that it comes from colibacillary elements, but we do not know from which animal they come.

We also know that colibacilli differ according to the strain and their origin, but we did not know that it is the animal species from which they come that is the cause of these differences. The colibacillus of the horse produces a lot of indol, that of the dog and herbivores much less. As with the races of tubercle bacilli, it is certain that the colibacillus of the horse differs specifically from those of the ox and of man, as proven by the virulence of the first two for the latter.

When soil is cultivated to obtain the tetanus bacillus, it is not the tetanus bacillus itself that is inoculated; it is the spores of an colibacillus of unknown origin, and it is these spores that, in anaerobic broth, will give rise to the tetanus bacillus. It therefore appears that by culturing human colibacillus in oxygen-free broth, it will be possible to obtain the tetanus bacillus or the septic vibrio, depending on the elements inoculated. A culture of a portion of the white layer that covers the blood cells of centrifuged blood, or a culture of a piece of connective tissue, if necessary chopped or finely scraped, can give the same result.

Let us now examine the origin of the culture used to prepare the anti-tetanus serum at the **Pasteur Institute.** I have sought to obtain precise information on this point; here it is:

It is always the same culture that has been used and is still used; it comes from **Metchnikoff's** laboratory; that is all that is known about it. We do not know whether it comes from an original earth culture or from pus from a human tetanus wound.

The antitetanus serum is therefore manufactured blindly, without knowing whether the culture used has the essential specific link with the human tetanus virus. The pasteurian school is not unaware, however, that an antitoxin has a specific action strictly limited to a specific autogenous strain. Since the aim was to protect against human tetanus, it was advisable to choose a culture of human origin.

If the culture was obtained with soil, it therefore relates to the colibacillus of an unknown animal species and, for this reason, antitetanus serum and tetanus toxoid can only be ineffective against human tetanus, which is autogenous.

The tetanus virus is autogenous for any animal when tetanus develops spontaneously; only experimental tetanus is heterogeneous.

The facts currently known formally demonstrate the autogenous origin of the tetanus bacillus. These facts are:

1. TETANUS FOLLOWING ASEPTIC SURGICAL OPERATIONS

Contamination by catgut has been alleged to explain this; this explanation is demonstrated to be inaccurate by tetanus following injections of quinine salts. In reality, these operations, when performed with complete hemostasis, create a deep wound and an anaerobic environment in certain areas.

2. TETANUS FOLLOWING ASEPTIC INJECTIONS OF QUININE SALTS

The determinism of this fact has not been achieved, but it is certain that, in this case, the spores that develop into tetanus bacillus at the injection site are those of the organic colibacillus, which exists everywhere, and not preformed and hypothetical latent tetanus spores.

3. TETANUS FOLLOWING SCALPING

Here, it is the obliteration of the testicular vessels by torsion that creates the anaerobic environment in which the spores of the organic colibacillus become tetanus bacillus. It is an operation equivalent to the long-held tourniquet that causes gas gangrene and the multiplication of the septic vibrio, another autogenous form of colibacillus.

Tetanus epidemics caused by soiled instruments used for surgical cutting are not proof against the spontaneity of tetanus. They only prove the soiling of poorly disinfected instruments after a previous or initial operation followed by spontaneous tetanus.

It is the total anoxyemia of a region that causes the transformation of the organic colibacillus into another tetanizing anaerobic form.

4. THE TETANUS BACILLUS IS IDENTIFIED WITH THE SEPTIC VIBRIO, ANOTHER AUTOGENOUS FORM OF THE COLIBACILLUS

It is identified by its transformation, on a solid medium, into a conidial form, *Corymbisporium*, identical in all respects to that obtained from the septic vibrio under the same conditions.

5. TETANUS TOXIN BROUGHT INTO CONTACT WITH OXYGEN TRANSFORMS INTO COLIBACILLUS

This is new formal proof that establishes that the tetanus bacillus of human tetanus is an anaerobic form of its organic colibacillus, because if it came from a heterogeneous colibacillus, its blood would contain an antitoxin.

It follows that human tetanus is autogenous and not produced by heterogeneous tetanus spores of external origin introduced into a wound.

6. ONE ATTACK OF TETANUS DOES NOT VACCINATE AGAINST A SECOND ATTACK

This is a fact that was already known before the discovery of serotherapy. It alone would be enough to prove the autogenic origin of tetanus. Indeed, diseases due to adulteration of the normal organelles constituting the organism do not vaccinate. Only heterogeneous viruses generally vaccinate (the malaria virus or viruses do not).

This fact, discovered by **Vincenzi** in 1893, constitutes formal proof of the autogenic nature of tetanus; it demonstrates that the tetanus bacillus is an autogenous formation of a constituent element of the organism, because, if this bacillus or its toxin is injected into a horse or another animal for which it is heterogeneous, it will develop an antitoxin; this is only ever developed by a microorganism that is heterogeneous for the organism in which it develops.

These last three conditions alone establish peremptorily that tetanus and the tetanus bacillus have an autogenous origin.

So, in summary, the autogenous tetanus bacillus of animals is a form of their organic colibacillus. The tetanus bacillus of man being a form of his organic colibacillus is therefore, like the latter, specifically different from the tetanus bacilli and colibacilli of animals and heterogeneous for them.

We will see later that this distinction is of capital importance to understand why the antitetanus serum from the Pasteur Institute has a protective action against experimental tetanus in animals caused by the same immunizing toxin and can have none against autogenic tetanus in humans.

Tetanus toxin transforms into colibacillus culture upon contact with oxygen in the air.

Tetanus toxin undergoes rapid modification when it comes into contact with air (see first volume p. 192).

We have known for a long time that, as Bacteriology treatises indicate, in order to preserve tetanus toxin, it must be deprived of contact with the oxygen in the air. We have therefore seen that it deteriorates upon contact with oxygen.

It is not an alteration that it undergoes, it is an evolution. Indeed: on contact with oxygen, the granulations of the toxin agglomerate and form increasingly larger round masses, then germinative masses emitting filaments which segment into bacilli of various lengths. This transplanted culture has an odor similar to that of tetanus and septic vibrio cultures; it produces indole. In this state it is a culture of septic vibrio evolving towards the colibacillus type.

Tetanus toxin is the filtrate of an anaerobic culture of tetanus. It is taught that this liquid contains soluble toxins produced by the bacillus. This is false; the complete culture of the bacillus in an anaerobic medium contains (see fig. 2 pl. 2 first vol.) isolated round spores, bacilli without spores, bacilli carrying a spore at their end, long and branched

mycelial filaments and germinative masses. The filtrate of this culture on a porcelain candle L 3 only contains the spores or micrococcal elements which are the active and filterable elements and which, when the toxin is brought into contact with the air, evolve and this time give, in aerobic culture, a bacillary form which is that of the colibacillus.

This constitution of the tetanus toxin being fixed, it results from its transformation into a colibacillus culture, when it is put in contact with oxygen:

1. That when we give the horse, to immunize it, subcutaneous, then intravenous injections of tetanus toxin, we introduce it into an oxygenated environment where it returns to the colibacillus type.

2. That, in any case, the action which develops the antitoxin or immunizing substance occurs in the blood of the animal, a highly oxygenated environment where the tetanus toxin becomes the colibacillus again.

3. That, under these conditions, it is an anti-colibacillus serum that is developed at the same time as an anti-tetanus serum. There can be no doubt about this, the tetanus bacillus or its toxin, which is the same thing, cannot survive in the blood, which is an oxygenated medium, and is transformed there into colibacillus.

Thus, in summary, the new knowledge of the existence of the organic colibacillus and its functions, which led to that of its identity with the septic vibrio and the tetanus bacillus, also leads to knowledge of the following conditions, previously unknown in the development of anti-tetanus serum.

1. Earth easily gives rise to a culture of septic vibrio or tetanus bacillus, because it always contains the elements of the colibacillus of humans and domestic animals, which are widespread everywhere;

2. The culture thus obtained in an anaerobic medium is that of the colibacillus of one or more unknown animal species;

3. The colibacilli of different animal species are specifically different;

4. If a tetanus culture comes from the human colibacillus and is used to prepare the toxin that will immunize the horse, the resulting serum will be anti-colibacillary for humans;

5. Tetanus toxin, whatever its origin, transforms and returns to its original colibacillary type if it is brought into contact with oxygen. This fact explains why the blood does not contain tetanus bacilli during the development of tetanus;

6. A tetanus toxin injected into a horse, which can only cause its preparative action (production of the antitoxin) in the blood, a highly oxygenated environment, transforms there into its original colibacillary type by which it acts by producing an anticolibacillary serum for the animal species from which the autogenous toxin originates; this serum is active against the antigen toxin in vitro and against experimental tetanus in other animals, but it is not active against their autogenous tetanus or that of humans. Indeed:

The serum, which is only anti-colibacillary for humans if the antigen comes from their autogenous tetanus, will indeed neutralize the tetanus toxin in vitro by agglutinating it, but will no longer neutralize it in the human organism, because the antitoxin will be completely absorbed by the enormous mass of the organic colibacillus; this explains the absence of therapeutic action of the anti-tetanus serum during the development of tetanus and demonstrates, in addition, the impossibility of a preventive action of this serum in humans due to the fixation of the antitoxin by the mass of the colibacillus.

The first injection into humans of anti-tetanus serum from a horse immunized with a tetanus toxin of autogenous human origin would be equivalent to a second unleashing injection because this serum would be anti-colibacillary.

Since the Pasteur Institute's antitetanus serum has caused fatal anaphylactic shocks at the first injection, it is precisely with a view to the mechanism of these accidents that I wanted to know the precise origin, the original source of the toxin injected into the horses supplying the serum. The **Pasteur Institute**, being ignorant of this origin, is therefore completely ignorant of the qualities of the serum it supplies, since it is ignorant of the name of the animal from which the antigen toxin necessarily comes, which is an anaerobic form of the colibacillus of an animal species; since the colibacilli of different animal species are all specifically different from one another, it is impossible to specify, under these conditions, whether the antigen toxin was of human origin, that is to say, autogenous, or of heterogeneous origin.

Now, this is a question of capital importance because an antitoxin is a body with a rigorously specific action, strictly limited to the strain of the antigen. This is a formally established fact, for example for different strains of colibacilli.

The antitetanus serum provided by an immunized horse therefore has completely different properties depending on the autogenous human or heterogeneous origin of the inoculated toxin.

It follows from these explanations that injecting humans with a horse serum prepared with a tetanus toxin of human origin is formally inadvisable, on the one hand because of possible accidents and on the other hand because it cannot result in any protective action, since autogenous tetanus in humans does not vaccinate them.

The results of experiments on experimental tetanus conducted on animals are not applicable to humans. Significance and criticism of these experiments.

The main evidence cited to prove the preventive and curative action of anti-tetanus serum is that it neutralizes the toxin in vitro and in vivo. A dose of tetanus toxin that is lethal to a guinea pig becomes harmless if it is mixed in vitro with a low dose of antitetanus serum. This lethal dose is also harmless if, 40 minutes before its injection into the guinea pig, it is injected with a certain dose of serum.

The serum acts in both cases because it agglutinates the formed elements of the tetanus toxin and thus renders them inactive, but it does not kill them or suppress their power to evolve. It should be noted that these results are only valid with regard to a single strain of toxin: the one that served as the antigen for the immunization of the animal providing the serum.

A serious error of reasoning was originally committed by **Behring** and **Kitasato** first, and then by those who studied the question after them, in considering the results of experiments on experimental tetanus in animals as applicable to humans. They made this error because they failed to see that spontaneous tetanus in humans and animals is a completely different case from experimental tetanus and because it escaped them:

1. That human tetanus is autogenous and does not vaccinate;

2. That the autogenous tetanus bacillus of an animal, inoculated into other animals, is for them a heterogeneous virus.

But it must be said that Pasteur's harmful dogma of the asepsis of animal organisms prevented them from thinking that a bacterial disease like tetanus could be autogenous. Moreover, they believed the tetanus bacillus to be a fixed, unique species; they were unaware of its origin, its nature, and that multiple and specifically different strains could exist.

New knowledge now allows us to completely clarify the question and to affirm that the conclusions that can be drawn from the study of experimental tetanus are not applicable to humans. We will examine the following two questions in turn:

1. Does the fact of the neutralization in vitro and in vivo, in animals, of the strain of toxin inoculated into the horse, by the antitoxin that it develops in its blood, prove that this same strain, inoculated into humans, will produce the same effect and immunize them against spontaneous and autogenous tetanus?

2. Does the same fact prove that the horse serum thus prepared acts in a preventive manner on humans to protect them against spontaneous autogenous tetanus?

The answer to the first question involves two cases: the first, where the strain of the tetanus bacillus and its toxin comes from a sample taken from a case of autogenous human tetanus, and the second, where the strain of the bacillus and its toxin comes from a soil culture.

In the first case of a tetanus bacillus of autogenous human origin, it can be stated categorically that inoculating the human with either the complete culture or its toxin will produce neither immunity nor antitoxin in the blood, that is, will not vaccinate. This is because it is known that a tetanus attack **does not vaccinate** and that, in **a man who has just been cured of tetanus, the blood does not contain antitoxin**.

The reason for this lack of vaccination lies in the fact that here, the tetanus bacillus is an anaerobic form of the organic colibacillus, a constituent element of the organism, and that vaccination never occurs against a constituent element, since the latter cannot form an antitoxin in the blood that would act against itself; diseases caused by deviations of the two constituent organelles never vaccinate.

In the second case, where the tetanus bacillus comes from a soil culture, its real origin is the colibacillus of an unknown animal species. In this case, the culture is heterogeneous for humans, and the injection of its toxin will produce an antitoxin in their blood at the same time as immunization against the colibacillus and the tetanus bacillus of the unknown animal species, but it will in no case be an immunization against autogenous tetanus in humans.

It therefore follows from this statement:

1. That in no case, whether in a man or an animal, can the injection of a tetanus toxin, whether of autogenous or heterogeneous origin, protect against autogenous tetanus;

2. That protection is only possible against heterogeneous tetanus, that is, against experimental tetanus. The infection of human wounds by earth containing tetanus spores is a gratuitous assertion that has never been confirmed by the slightest evidence. The production of tetanus by inoculation with earth does not prove that this is the necessary and sole source of tetanus.

As for the second question relating to the protection that antitetanus serum can confer on humans, the two cases examined above must also be considered.

In the first case, if the antitetanus serum comes from injecting the horse with a toxin that is heterogeneous for humans, it cannot protect it against its specifically different autogenous tetanus.

In the second case, if the antitetanus serum comes from autogenous human toxin, it does contain an antitetanus antitoxin, but it will only act to protect an animal against experimental tetanus due to the human toxin. This antitoxin injected into humans will have no effect because it is anti-colibacillary for them, the toxin inevitably reverting to the colibacillus type in the horse's oxygen-rich blood where the antitoxin is produced.

Under these conditions, the antitoxin will be completely fixed by the considerable mass of the colibacillus of the man injected with the antitetanus serum, and it will not be able to exert the slightest action on the tetanus bacillus, its quantity being negligible compared to the mass of the organic colibacilli.

Thus, whether it is toxin or antitetanus serum, neither can exert any action on humans for the reasons just indicated.

It was stated above that the **Pasteur Institute** has been completely ignorant of the origin of the tetanus culture it has been using since the beginning of production. It is therefore not possible to indicate in which cases tetanus toxoid and antitetanus serum fall.

But we see that, ultimately, this ignorance is of only secondary importance since, since human tetanus is autogenous, does not vaccinate and does not determine the formation of antitoxin, we can have no control over it, neither as a preventative measure by toxoid, nor from a curative point of view by the injection of a serum.

* * *

If we summarize the facts presented so far and if we clarify their significance, we see that antitetanus serum therapy was a big mistake:

1. Because immunization of animals against tetanus with antitoxin production can only be achieved with a tetanus bacillus that is heterogeneous to the inoculated animal, that is, in the case of experimental tetanus;

2. Because spontaneous tetanus is always autogenous in animals, does not immunize them and does not develop antitoxin;

3. Because the tetanus bacillus arises spontaneously by transformation of the organic coli-bacillus at a point in the body that has become abnormally anaerobic, and because the preservation, by the tetanus bacillus, of the specific colibacillary quality excludes

any possibility of vaccination. Diseases due to an alteration of a normal constituent of the body do not vaccinate.

Can the injection of tetanus toxin protect humans against autogenous tetanus?

It was already known that this protection was impossible when serotherapy began to be applied, since it was known that a tetanus attack does not vaccinate, and since it had been known since 1893, through **Vincenzi's** observations, that the blood of a man who has just been cured of tetanus does not contain antitoxin.

This book also makes known:

1. That tetanus is autogenous, as is the tetanus bacillus;

2. That tetanus antitoxin only develops if tetanus is heterogeneous and developed by experimental inoculation;

3. That the tetanus bacillus develops in the animal organism by transformation of the organic colibacillus into a point that has become anaerobic and that this autogenous development does not involve the possibility of vaccination against tetanus;

4. We have seen above that in no case can tetanus antitoxin, whatever the antigen that develops it, prevent the outbreak and development of autogenous tetanus in humans and animals.

All these facts constitute peremptory and absolute proof of the total impossibility of obtaining the slightest protective action in humans by an injection of tetanus toxin.

Let us add to this that the **Pasteur Institute** is completely ignorant of the origin and quality of the toxin with which it recommends injecting children.

It is therefore without the slightest proof and contrary to the truth that the **Pasteur Institute** asserts the immunizing properties of its toxoid.

It is therefore inconceivable that children could be inoculated with, and even more inconceivable that they should have made it compulsory to inoculate them with, a toxin that is completely devoid of any pre-immunizing action.

It is therefore also inconceivable that millions of men have been injected over a long series of years with a serum claimed to be anti-tetanus and pre-immunizing, when it has been proven, since 1893, that it is completely devoid of any pre-immunizing action, since it is known that a tetanus attack does not confer immunity.

Since this injection of serum also causes serum sickness and exposes people to serious anaphylactic accidents, and since it is also strictly impossible for serum and toxoid manufacturers to provide proof of their curative or preventive action, which cannot exist since tetanus is autogenous, does not immunize, and does not form an antitoxin, it becomes necessary to prohibit the inoculation of children with tetanus toxoid, which no law has made mandatory until now, as well as the injection of antitetanus serum as a preventive measure.

Moreover, even if these two products were active, it would be a serious and unacceptable error to inoculate humans with the colibacilli of horses and unknown animals with antitetanus serum or toxoid to protect them from the extremely minimal risk of contracting tetanus.

Thus, during the years 1936, 1937, and 1938, the number of tetanus cases was, on average, approximately 1 per 400,000 individuals in the British Isles and 1 per 12,000 in Romania, the country where tetanus is most common.

In *Errors and Deceptions of Modern Medical Science* (1910), Doctor **Bourget** reports (pages 73 and 74):

1. That Doctor **Secrétan**, who never gave preventive injections of antitetanus serum, claims that, out of approximately 12,000 wounded people treated at his outpatient clinic, including a good number of stable hands, he never saw a single case of tetanus;

2. That Doctor **Rieffel** reported to the Société de Chirurgie (May 15, 1907) that he does not believe that antitetanus serum has the slightest effect on the development of tetanus, even in slow-onset forms;

3. That Professor **Berger** was able to collect 35 cases of tetanus developed **despite preventive injections.**

CONCLUSIONS

1. Soil gives rise to cultures of tetanus bacillus and septic vibrio because it contains the excremental matter of domestic animals and humans, notably the elements of their organic colibacillus;

2. Such a culture of tetanus bacillus or septic vibrio therefore emanates from an undetermined animal species or from humans. However, the colibacilli of different animal species are specifically different and the colibacillus of one species is virulent for the others;

3. The antigen quality of a tetanus culture obtained with soil is therefore unknown, since it is unknown to which species of animal it corresponds; the tetanus culture used by the Pasteur Institute is in this case, since it is completely ignorant of its origin;

4. Tetanus toxin is the filtrate on a porcelain candle of a tetanus culture developed in an anaerobic environment. This toxin does not evolve if it is protected from air. If, on the contrary, it is brought into contact with oxygen, it evolves and transforms into a culture of another bacillary form, colibacilli.

This fact establishes that the tetanus bacillus is a form of anaerobic transformation of the organic colibacillus of animals; it also demonstrates that the development of the tetanus bacillus is spontaneous in animals and occurs by transformation of the form of colibacillus in an anaerobic environment.

5. Thus are explained:

A) The presence in the tissues of latent tetanus spores that has been mentioned: these are the countless quantities of colibacillus spores or cocci that exist everywhere.

B) Tetanus following bistournage.

C) Tetanus following injections of quinine salts.

D) Tetanus following aseptic surgical operations;

6. In addition, to the fact that tetanus is autogenous, it does not vaccinate the person affected. **Vincenzi** (1893) has in fact verified that the blood of a man who has just been cured of tetanus does not contain antitoxin;

7. These various conclusions are in formal disagreement with those which, since the work of **Béhring** and **Kitasato**, have constituted the fundamental bases of antitetanus serotherapy, which are the following:

A) The tetanus bacillus is a heterogeneous species for animals and humans.

B) Tetanus developed in an animal by inoculation with tetanus toxin or a complete culture immunizes it against subsequent inoculation with a lethal dose of the same toxin.

C) The serum of the immunized animal contains an antitoxin that neutralizes in vitro and in vivo one or more doses of toxin that are fatal to a new animal.

These facts, although perfectly correct, have been misinterpreted because it has been ignored:

A) That the development of the tetanus bacillus and tetanus is always autogenous in humans and animals.

B) That there exists in every animal a bacterial or colibacillary function and that the tetanus bacillus of each species is an anaerobic form of its organic colibacillus.

C) That there are as many races or species of tetanus bacilli as there are animal species.

D) Let us add to this that it has been forgotten that antitoxins are bodies acting by their agglutinating action on the antigen and that their action, rigorously specific, is strictly limited to the antigen that develops them and to the animal species from which this antigen originates;

8. It is because these facts have been ignored that it has been falsely admitted that the conclusions drawn from the study of experimental tetanus, in which the tetanus bacillus is heretogenic, can be applied to spontaneous tetanus in humans and animals where it is autogenous.

This error leads to the following necessary corrections:

A) The conclusions resulting from the study of experimental tetanus in animals are not applicable to spontaneous and autogenous tetanus in humans and animals.

B) The antitoxin developed in the blood of an immunized horse (antitetanus serum) cannot have any effect on autogenous tetanus in humans and animals because it is developed by a tetanus bacillus of a different species from the human tetanus bacillus or from that of an animal that contracts tetanus following castration.

C) Even if the horse were immunized with the toxin of the autogenous tetanus bacillus of humans, the injection of its serum could have no effect on the course of autogenous tetanus in humans, because the small amount of injected antitoxin would be immediately fixed by the considerable mass of the organic colibacillus and thus rendered inactive.

Thus, under no circumstances, regardless of the origin of the antigen used to manufacture the antitetanus serum, can it exert any preventive, much less curative, action in humans.

D) Consequently, since the injection of this serum creates a state of anaphylaxis and the possibility of various accidents, and since it inevitably inoculates humans with equine colibacillosis, a disease that lasts as long as the state of anaphylaxis, the use of this serum must be rejected and should even be prohibited.

E) As regards tetanus vaccination with **Ramon's** toxoid, a first conclusion is that it cannot in any way have the slightest effect on autogenous tetanus in humans since it is made up of a tetanus bacillus of a different species from the tetanus bacillus of human autogenous tetanus. In fact, the **Pasteur Institute** is unaware of the origin and anti-

genic quality of **Ramon's** toxoid, that is to say the name of the animal of which it is a form of the colibacillus. If it comes from a tetanus bacillus obtained by soil culture, this is therefore almost certainly heterogeneous for humans. According to the rule, this heterogeneous toxin for humans will infect them by communicating acute and chronic colibacillosis of the animal from which it comes and will immunize them against the tetanus bacillus and against the colibacillus of this animal but, in no case, against autogenous tetanus of humans.

9. If this toxoid came from autogenous tetanus of humans, its inoculation in humans could not vaccinate either since an attack of tetanus does not vaccinate. But it would at least not have the disadvantage of inoculating humans with colibacillosis from an animal;

10. Autogenous tetanus of humans does not immunize them because its toxin, when passing into the blood, an oxygenated medium, returns to the colibacillary state, that is to say, to the state of fibrin-ferment which cannot form an antitoxin against itself;

11. A tetanus attack cannot immunize a human because upon recovery, the tetanus bacillus disappears from the body and returns to its original state of colibacillus;

12. Current vaccinations, which simultaneously and surely cause:

1)the chronic phase of diphtheria by diphtheria toxoid;

2) colibacillosis of an unknown animal by toxoid and antitetanus serum;

3) colibacillosis of horses when it comes to antitetanus and antidiphtheria serums, should be prohibited because the use of these harmful serums and vaccines, whose therapeutic action is null, constitutes a national danger against which it is appropriate to protest.

To conclude, I refer the reader to the conclusions of Chapter XIX relating to compulsory vaccinations.

CHAPTER XIII

STUDY OF TUBERCULOSIS

THE DUMBBELL ORGANELLE OF CERTAIN CELLS OF THE ORGANISM CAN, UNDER CERTAIN ABNORMAL CONDITIONS, TRANSFORM INTO KOCH'S BACILLUS THROUGH DEGENERATION AND EXACERBATION OF ITS POWER OF MULTIPLICATION

In the second volume of this work, it has been demonstrated:

1. That Koch's bacillus is the dumbbell organelle that constructs the cells of the organism, modified in its vegetative properties by degeneration;

2. That the development of tuberculosis in the human lung results from that of embryonic cells. The first of these are formed by the dumbbell organelles of desquamated epithelial cells and by those of the wall of the pulmonary alveoli.

The first embryonic cells germinate and give rise to dumbbell filaments articulated end to end by their balls which, insinuating themselves into healthy tissues, cause tuberculous infiltration.

The filaments thus formed become in turn capable of giving rise to new embryonic cells which germinate like the first, emitting new dumbbell filaments. This cycle of evolution continues until the complete destruction of the pulmonary parenchyma or until the exacerbation of the dumbbell's multiplying power is extinguished, a fact which occurs by a mechanism unknown to us but which, presumably, consists of the same phenomenon as that which, in the organism, maintains the dumbbell organelles in a state of stability in the filaments or in the cellular networks they have formed.

This arrest phenomenon coincides exactly with the transformation that **Calmette** and **Guérin** achieved with bovine Koch's Bacillus by cultivating it in a bilious medium, which causes it to lose its tuberculizing power and returns it to the state of a non-tuberculizing organelle, that is, normal or close to normal.

This loss of tuberculizing power is not, as previously thought, an attenuation of the virus, because an attenuation is never stable and can easily disappear through in vitro culture, or change of medium, or serial inoculation.

On the contrary, the loss of tuberculizing power appears to be fixed and hereditarily transmissible.

This phenomenon is of considerable importance from the point of view of research into ways to stop the progression of tuberculosis, since culture in a bile medium is the first example of a process capable of causing the dumbbell organelle, which has become Koch's bacillus, to lose its tubercular property and return it to the state of a normal dumbbell organelle.

This is the goal of the treatment of tuberculosis. The experiment of **Calmette** and **Guérin** is of great importance because it demonstrates that, by a change of medium, the properties of **Koch's** bacillus can be completely modified and that the normal elements of bile are capable of effecting this change.

* * *

According to **Borrel**, embryonic cells would be lymphatic cells that have left the vessels by diapedesis and by the activation of a defense process of the organism. This assertion has not been supported by any specific fact; it has been demonstrated to be inaccurate:

1. By the different constitution of the two types of elements; embryonic cells contain only dumbbells, while leukocytes contain only granules distinct from each other;

2. By the fact that embryonic cells, often pyriform, are always pedunculated when they are young, while blood leukocytes never are;

3. By the fact that the dumbbell balls of embryonic cells give rise either to Koch's bacilli or to filaments of Koch's bacilli placed end to end, a fact which is exactly the opposite of **Borrel's** assertion, according to which leukocytes have the function of phagocytosing, that is, of destroying Koch's bacilli. On the contrary, these cells, which **Borrel** considered to be lymphatic, give rise to Koch's bacillus**!**

The nature and origin of Koch's bacillus are definitively established, beyond dispute:

1. By the fact that, having the shape of a dumbbell, it is an elementary organelle already structured and not a simple bacillus;

2. By the fact that, having the shape of a dumbbell, it originates from a ball of a dumbbell of an embryonic cell which, itself, originally, results from the growth and multiplication of a dumbbell of a cell of the affected organism.

Knowledge of the nature and origin of Koch's bacillus brings new facts that were not suspected. When the evolution of tuberculosis stops definitively, all Koch's bacilli in the invaded region therefore return or can return to the state of normal organelles.

This invaded region also contains the second constituent element of the organism, that is, the second elementary organelle of the living being: the colibacillary granulation,

capable of reorganizing, at least in part, the altered region and of determining the formation of the fibrous tissue that will sclerosis the lesion. This capacity of the granular elements of the colibacillus was formally demonstrated during the study of the coagulation of fibrinogen solutions (page 219). I recall here that I succeeded in transforming:

1. Normal cultures of human tuberculosis into hyphomycetes of the conidial forms *Penicilium, Sterigmatocystis, Aspergillus minimus* (**Bainier**), *Mucor*, and *Monosterygmate* **(1)**;

2. Cultures of bovine tuberculosis, in conidial forms: *Sporothricum, Penicilium, Aspergillus* (*Glaucus* type), *Mucor,* the latter very clearly different from the *Mucor* form of the human tuberculosis culture;

3. Cultures of avian tuberculosis in *Penicilium*, *Sterigmatocystis* and *Monosterygmate.*

The three *Sporothricum* forms are very different from each other.

The differences in these conidial forms between the three types of tuberculosis are so marked that they prove a very marked specific difference between them. The production of these various transformations was explained in 1926 in the first volume of this work.

* * *

Professor **G. Enderlein** published in 1931 (18 A) a long memoir in which, after having explained that he had succeeded, as I had done long before him, in transforming tuberculosis cultures into hyphomycetes of the forms *Aspergillus flavus*, **Link**, and *Aspergillus fumigatus*, he sought to demonstrate that Koch's bacillus is a form of development of hyphomycetes of the genus *Aspergillus* and that this is the direct agent, the causal agent of the development of tuberculosis.

He saw that, in hanging drop cultures, *Aspergillus* spores give rise to fine filaments ending in a ball that he calls chondrite, the name symprotite being given to the ball alone. This can in turn give rise to a new filament ending in a ball, and so on. At this point, the culture is subcultured onto agar and colonies of the same elements develop, which, within a period of 11 days to a few weeks form other, much smaller ones, corresponding to a stage called atrophic basit by **Enderlein**. The elements thus formed would be acid-fast and would correspond to the stage of virulence of Koch's bacillus.

The facts presented by **Enderlein** therefore have no connection with the development of tuberculosis. He erred in concluding that, due to the transformation of Koch's bacillus into *Aspergillus*, there is a specific identity between them and that, consequently,

(1) *I called this form Monosterygmate in the first volume, a form which, in my mind, contains four or five genera that have received different names for no plausible reason, for example, Torula, Oidium, Acremoniella, and other cataloged names.*

Aspergillus, is indeed the active agent, the tuberculosis virus, an active agent that he indicates as being of heterogeneous origin, vegetating on the most diverse environments, food materials, detritus, etc., and whose spores, extremely widespread in the external environment, would be the cause of tuberculosis infection.

In publishing these facts and conclusions, **Enderlein** was certainly unaware of the existence of the first volume of *Constitution of Animal and Plant Organisms*, because if he had known it, he would have known that the specific link that exists between Koch's bacillus and the *Aspergillus* into which it transforms in no way means that the latter is the agent of tuberculosis.

He would have seen, in fact:

1. That normal living matter from all parts of the healthy organism can give rise to the *Aspergillus* form as well as to *Koch's* bacillus;

2. That I have obtained the transformation of Koch's bacillus into conidial forms, *Penicilium*, *Stematocystis, Aspergillus minimus, Aspergillus glaucus*, into two very different species of *Mucor*, etc., and, consequently:

A) That the different conidial forms, all originating from Koch's bacillus, would be as legitimate a form of the tuberculosis virus as the *Aspergillus* form.

B) That they are only, in reality, the expression of the extreme polymorphism of living matter, and that the latter, with each change of form, acquires new properties, different from the previous ones. He would have seen, moreover, that I had already concluded (page 426, first vol.): that the tubercular element is the organic mold of the species, deviated in its form and that the animal organism carries within itself the element necessary for the spontaneous development of tuberculosis.

In summary, **G. Enderlein** made a fundamental error in concluding that species of *Aspergillus* are the tuberculosis virus. He succeeded, moreover, neither in obtaining typical tuberculosis cultures from them, nor in causing typical tuberculous lesions in animals.

In a chapter of his memoir entitled *Consequences of the Specific Identity of the Tuberculosis Bacillus and the Aspergillus Mold*, he indicates that the fight against tuberculosis must consist of the destruction of this mold and its spores, which exist everywhere and, spread in the air, infect the respiratory tract, and he concludes:

The fight against the Aspergillus mold, being the cleanest and most accessible moment in the history of the development of the tuberculosis producer, is sufficient to guarantee a progressive and safe destruction of the contagion.

This conclusion is false, as *Aspergillus* plays no part in the development of tuberculosis, which results exclusively from an abnormal evolution of the tissue-building dumbbell organelle leading to the formation of embryonic cells, then of Koch's bacillus through the balls of the dumbbells that contain the latter.

In a third memoir, **Enderlein** indicated in 1933 that I had obtained, in 1926, the transformation of Koch's bacillus into the hyphomycetes of the conidial forms *Penicilium* and *Aspergillus minimus* **Bainier,** an indication that his 1931 memoir did not contain, evidently because he was unaware of the existence of the first volume of this work. But, in this quotation, he only mentioned one *Penicilium* form and the *Aspergillus minimus* **Bainier** form, although I reported having transformed the human, bovine, and avian bacilli into a second *Aspergillus* form, then into the *Sterigmatocystis* form and into two very different forms of *Mucor.*

Villemin's experiments did not demonstrate the heterogeneous origin of the tuberculosis virus or the contagiousness of tuberculosis.

I have already explained, in the second volume of this work, that the conclusions drawn by **Villemin** from his experiments considerably exceeded their significance.

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His experiments signified exclusively that the matter which constitutes the tuberculous lesions and in which he rightly located the virus of the disease, reproduces tuberculosis by inoculation. They did not mean that the virus is of external origin, that is to say, heterogeneous, nor that tuberculosis is contagious, nor that it cannot develop spontaneously outside of any contagion, nor that heredity does not play an important role in the development of the disease, as he falsely concluded.

As we shall see, these conclusions not only had the effect of halting the future progress of science, but of setting it back significantly by contributing, through an experiment that was only apparently decisive, to the establishment of these harmful dogmas of atmospheric panspermism, of the asepsis of living organisms, of contagion as the alleged cause of their infectious diseases.

Here is how **Pidoux** judged **Villemin's** conclusions in 1865 (59):

For Mr. Villemin, says Pidoux, tuberculous phthisis of the lungs is virulent and contagious, virulent and specific like syphilis, virulent and contagious like Farcin glanders: to demonstrate this, it was first necessary to prove that this disease cannot develop instantaneously or through common determining causes; but, as the mass of the best-known facts presents itself insurmountably to him, Villemin denies squarely and in principle that living organisms, and their elements, are and can be spontaneously alterable. Nothing, says Villemin, can put man in the condition to produce tubercle, it must therefore come to him from outside. Now, as, according to the same author, common

causes from outside, to whatever order they belong, heat, cold, dryness and humidity, excess or deprivation of the elements of hygiene, do not bring to man the ready-made tuber, and that they can only put him in the condition to make one, it is necessary that the organism is only the recipient of this tuber whose seed arrives to it ready-made from outside. As in inoculations to rabbits, it is necessary that the tuber is born only from the tuber, this is the simple doctrine, down-to-earth doctrine of specificity applied to tuberculosis. For all diseases like measles and scarlet fever, the lineage is not difficult to follow; their virulence and specificity, consequently, were recognized at once, because they are obvious and one does not need to be a scholar to observe them: on the contrary, it is as the study of Medicine has entered the scientific paths of rigorous observation that the belief in the contagiousness and specificity of tuberculosis in general and of pulmonary phthisis in particular has gradually died out.

Fascinated by his inoculations, and unwilling to believe that tuberculosis could occur in humans other than in his rabbits, Mr. Villemin begins by denying diatheses and any morbid spontaneity of the organism; he soon removes from the animal even the slightest physiological spontaneity, and thus forces himself to make it come from outside Mr. Villemin crowns his doctrine by proclaiming a nosological panspermia and by making the germs of tuberculosis and all diseases float in the clouds. If Mr. Pasteur needs new nosological panspermia proofs to support the doctrine of unlimited homogeny, this theory will provide him with innumerable and very unexpected ones. I believe, however, that pathology is a field where the truer and more philosophical doctrine of spontaneous generations or heterogeneity could gather many precious facts: pathology is only the knowledge of the heterogeneities to which the organism is subject, diseases are only heterogeneities, and tuberculosis is an example, it is a regressive heterogeneity or a spontaneous degeneration... in physiology, in pathology, evolution is everything, and heterogeneity or spontaneous morbid generation is therefore in the deviation... The most diverse and most opposed external causes determine the tubercle, everything is an occasion for it as it comes from us, as it is indeed one of the products of the specific and spotan alterability of our organic elements... The tubercle is one of the most banal heterogeneities; if many non-specific causes determine its spontaneous generation, a greater number prepare it, and these are the least occult and most natural causes, causes that have nothing seminal. In a very large number of cases, one can touch them with one's finger, and see the dreaded, therefore predicted, phthisis born and developing without any specific or contagious intervention... No one will dispute, in fact, that tuberculous phthisis is among the organic diseases of diatheses. Mr. Villemin completely rejects diathesis and almost completely heredity... All acquired notions are overturned, we must accept from one day to the next that phthisis falls from the clouds, and that in its pathogenesis, the subject, his constitution, the hygienic conditions, heredity, the diatheses, are nothing, and that everything is on the blade of a lancet, loaded with impossible tubercular virus, coming without doubt from a tubercular who got it from another, and so on, up to the first man,

who nevertheless got it from no one, and must have formed it from scratch, but something very extraordinary, without transmitting this fatal property to his children.

Thus, the belief in the contagion of tuberculosis, which existed in the first half of the 19th century and which the observations of the great clinicians of that time had gradually caused to disappear, was reestablished suddenly in 1865 by the sole fact of an erroneous conclusion by **Villemin** drawn from an experiment in subcutaneous inoculation carried out under conditions which had no relation to the normal living conditions in which the contagion of tuberculosis would be likely to occur.

Even today, this false notion persists; contagion must, as I indicated in the second volume of this work, be considered possible in principle but reduced to rare cases, the spontaneous development of the disease being the rule in subjects, whether or not they are in the power of heredity.

Thus, in 1865, Pidoux wrote:

Tuberculosis is a regressive heterogeneity or spontaneous degeneration... in physiology, in pathology, evolution is everything, and heterogeneity or spontaneous morbid generation is therefore in deviation.

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What clairvoyance and what admirable accuracy of vision. There is not a sentence, not a single argument in **Pidoux's** long protest against **Villemin's** conclusions that is not currently justified.

Later, the discovery of Koch's bacillus came, in appearance only, to provide a resounding confirmation of **Villemin's** conclusions, which, from that moment on, were no longer contested.

But, in 1926, a first study (the first volume of this work) established the bacterial nature of living beings and demonstrated not only that the pasteurian dogma of the asepsis of living organisms is false, but that, on the contrary, these organisms are the original source of all viruses, these being either autogenous for diseases with spontaneous development, or heterogeneous and of external origin in the case of infectious diseases; in the latter case, the heterogeneous virus is most often one of the foods of humans and animals and, in most cases, one of their plant foods.

In 1936, a new study, that of the second volume of this work, devoted exclusively to tuberculosis, established the perfect accuracy of the nature or origin that **Pidoux** attributed to it by demonstrating that Koch's bacillus, which had until then been believed to be a heterogeneous bacterial species, is the dumbbell organelle that builds the body's tissues, spontaneously modified in its properties, and having acquired, by spontaneous degeneration, as **Pidoux** wrote, its tubercular property.

The proofs I have given of the filiation of Koch's bacillus with the dumbbell organelle are peremptory. We see embryonic cells, clearly formed by dumbbell organelles, giving rise either to isolated Koch's bacilli or to bundles of parallel bacillary filaments.

The multiplying property of the dumbbell organelle is not in doubt, since it is its principal property, the one that allows it to construct anatomical elements, cells, nerve fibers, etc. Once these elements are constructed and have become adults, the multiplying property temporarily shuts down and usually only awakens during cellular multiplication or nuclear division.

This state of rest of the dumbbell organelle is determined by the constitution of the normal environment in which it lives. But if the conditions of this environment become abnormal—for example, if plasma circulation is suppressed by a specific cause or if certain cells are isolated from their neighbors by trauma, for example—the dumbbell organelle, whose environmental conditions are changed, recovers its initial multiplying property, which, exacerbated, is exerted this time without limit and manifests itself by the creation of embryonic cells and the formation, by these, of skeins of parallel bacillary filaments which are the agents of tuberculous infiltration.

Proof of the possibility of the spontaneous degeneration of the dumbbell organelle into a tuberculizing organelle was made by **Calmette** and **Guérin** by transforming the bovine tubercle bacillus, by culture in a bile medium, into another bacillus which no longer tuberculizes: it has returned to its normal dumbbell organelle state or to a very similar state.

Thus, in summary, the precise determinism of the nature and origin of the Koch bacillus, of the embryonic cells and of the evolution of these have therefore led to note both the accuracy of **Pidoux's** criticisms and the spontaneous development, in the patient's own organism, of the virulent or tuberculizing property of the dumbbell organelle, that is to say of tuberculosis, a fact which, demonstrating **Villemin's** error, agrees with the observations of the clinicians of the last century who concluded that tuberculosis was not contagious.

The tuberculosis virus is born spontaneously in the human body or in that of cattle, reproduces tuberculosis by inoculation and perhaps by ingestion in massive quantities through the digestive tract, but this does not prove contagiousness by contact between individuals under the usual conditions of life.

It is the same for other viruses such as those of miliaria and childhood poliomyelitis which only contaminate by ingestion of virulent food and not, as has been clearly observed, by contact between individuals. This is a probably general rule which calls for the conclusion that epidemics are not the result of contagion between individuals as dog-

matic bacteriology teaches, but the result of a simultaneous infection of a large number of individuals by the ingestion of the same food virus.

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Is the creation of immunity against tuberculosis possible?

Immunity consists of the fact that after a second infection with the same virus, there is generally a considerable shortening and attenuation of the bacterial phase or acute phase of infectious diseases, which occurs by accelerating the passage of the virus from the bacterial state to the mycelial state; this characterizes the state of the virus in the chronic phase of the disease, a phase which also exists, with its disorders or sequelae, in the case of immunity induced by inoculation.

In the case of tuberculosis, the development of the disease does not take place, at the beginning, by a phase of bacterial multiplication; the development is from the outset of a mycelial nature; it is chronic from the beginning because Koch's bacillus is not a mic- robe in the usual sense of the word, but an already differentiated organelle, a tissue builder.

As it develops, it does not form isolated bacteria, but filaments of dumbbell organelles and cellular elements, embryonic and epithelioid cells, in the most active phases of tuberculosis development. Therefore, there is no room in this development for a transition from the bacterial state to the mycelial state. For the tubercle bacillus, the filamentous form is the mycelial form, and it takes this form immediately from the beginning of the development of the first tubercle or infiltration because there is not the slightest difference, morphologically, between a filament of Koch's bacillus and a long neurofibril of the nerve centers or the axis-cylinder of a myelinated nerve fiber.

It is because of this nature of constructing organelle and because of its filamentous or mycelial form that immunization against Koch's bacillus cannot exist.

In a disease like typhoid fever, the acute and dangerous period is the bacterial phase. As soon as this period ends, the disease enters the mycelial phase, a chronic, extremely long, and still little-known phase that coincides with the establishment of immunity.

For tuberculosis, on the other hand, it is the mycelial phase that develops the dangerous lesions, and it is absolutely the same for the development of cancer. This explains the successive failure of all attempts at anti-tuberculosis immunization to date, a failure due to the fact that by inoculating a tuberculosis virus or a cancer virus, the mycelial phase of tuberculosis and cancer is immediately developed.

By inoculating cattle with certain strains of weakly virulent bacilli, it has been possible to create a certain degree of resistance against a subsequent experimental tuberculosis infection, but this resistance does not last long, and it is ineffective against a somewhat severe inoculation.

Another disease, syphilis, is caused by a hyphomycete that I isolated (p. 622, pl. 283, first vol.) and whose mycelium bears an innumerable quantity of spirillary appendages which are *Treponema pallidum.* It is because of this mycelial nature that syphilis does not immunize and that its lesions actively develop for decades. This disease constitutes a formal demonstration of the impossibility of immunizing against a pathogenic agent that infects the organism in the mycelial state, at the very site of its original penetration.

Can the immunity that inoculation with attenuated tubercle bacilli is powerless to create be obtained by a bacillus which, like BCG, has lost its tubercular faculty? This is what we are going to study.

* * *

In certain diseases, such as scarlet fever, which has an acute and febrile period, there is a bacterial phase with the development of bacterial and mycelial elements in the blood.

This phase is followed by an exclusively mycelial phase, which I demonstrated by photographing the mycelial elements contained in the blood (see scarlet fever, first volume).

Now, the disease generally immunizes, but immunization, which is always acquired after the end of the acute period, is ineffective with regard to lesions of the chronic period, or mycelial phase, a long-term period which follows the acute period. Indeed, renal lesions (scarlet nephritis) continue to evolve for several years despite the state of immunity.

This means that immunity can be created against the acute, bacterial phase of the disease, but it is ineffective against subsequent mycelial vegetation, precisely because it is this mycelial vegetation that creates immunity by developing in the blood the fibrinous, agglutinating substance (agglutinin), which causes the bacterial elements to pass into the mycelial state.

This long-lasting chronic or mycelial period exists in all infectious diseases, and it seems quite likely, given the still very early stage of knowledge of diseases and their chronic phase, that some of them, affecting organs such as the liver, kidneys, blood vessels, nerve centers, etc., are linked to the mycelial phase of distant previous infections, such as eruptive fevers, typhoid fever, certain uncatalogued tonsillitis that are followed by a very serious, long-lasting, and generally fatal chronic nephritis, etc., or are sequelae of the chronic phase that follows the inoculation of vaccinia with the aim of provoking immunity.

All these facts demonstrate the impossibility of vaccinating against mycelial viruses. However, the chapter on smallpox-vaccine formally demonstrates the mycelial nature of these two viruses; what is inoculated in smallpox vaccine is a product of the grinding of the mycelium and conidia of the hyphomycete, the agent of vaccinia, which can be seen photographed in plates 71 and 72.

There is, in appearance, a contradiction here, since the inoculation of these mycelial products achieves immunity. This is only an appearance, since immunity consists precisely in the passage from the bacterial phase to a mycelial phase, and because, in cases of natural contagion of smallpox, it is a bacterial phase that is the cause of the high temperature at the beginning of the infection.

The virus infects the organism in the hyphomycete form through its conidia, but these cause not only mycelial development, but also bacterial development, as has been demonstrated for diphtheria.

Finally, in the current state of medical knowledge, the primary, original cause of certain diseases, chronic from the outset, with slow progression such as chronic nephritis (**Bright's** disease), arteriosclerosis, certain states of hepatic deficiency, etc., has remained completely unknown; is it not linked to the slow and progressive mycelial development of viruses such as those of eruptive fevers, typhoid fever, and vaccinia?

We already know about the chronic nephritis of scarlet fever, those of certain still unknown viruses that cause a common angina, leaving in its wake a slight albuminuria, the first sign of chronic nephritis that is fatal after several years; we also know about the nephritis of syphilis, due to the development of the hyphomycete which is the infecting agent of the original chancre (which I isolated, cultivated and described; see p. 622 of the first volume) on which develops an innumerable quantity of tendrils similar to those of *Trichophyton gypseum* **Sabouraud** and which are *Treponema pallidum*.

It is certain that other viruses produce, in the long term, mycelial lesions. Jennerian vaccinia is one of them, as well as diphtheria and the antidiphtheria toxoid vaccine.

* * *

Messrs. **Calmette** and **Guérin** have succeeded, by culture in a bile medium, in obtaining a breed of bovine Koch's bacilli that has lost the tubercular property, but not all virulence, a completely distinct thing. This is a very remarkable acquisition, and we must recognize the great merit of the two scientists who obtained it through long work with the sole aim of achieving the elimination of a scourge such as tuberculosis.

But, while placing very high value on their discovery, the meaning of which is very different from that which they gave it, as we will see later, it nevertheless remains subject to scientific criticism, which should not be considered as a systematic denigration when it only uses serious and justified arguments; this being said so that what follows can in no case be considered as such.

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The application of **Calmette** and **Guérin's** discovery to anti-tuberculosis vaccination has been vigorously opposed by various scientists, notably by **Lignères**.

Anyone who has carefully read the book in which **Lignères** summarized all his interventions relating to the properties of BCG (42) and who has impartially examined the arguments brought forward on both sides in the discussions he engaged in with **Calmette**, inevitably becomes convinced of the accuracy of his criticisms and views on this vaccine, of his perfect loyalty in his interventions, and of the considerable importance of the role he played, in the general interest, by entering the arena alone, as he himself says, to discuss the efficacy and properties of BCG. In acting thus, through the calm and moderation he was able to maintain in often difficult conditions, he demonstrated a rare independence of mind and courage, which are the attributes of a very high character and dignity. This opinion of this eminent scientist results only from an impartial examination of the facts and his work, for I did not know him.

* * *

Examination of the facts has convinced me that the application of the BCG vaccine to humans was very premature, that is, it was not preceded by the production of the necessary evidence establishing its efficacy and safety. In such a serious matter, experiments conducted on guinea pigs and other animals are insufficient to allow for application to humans.

It must not be forgotten, when one wishes to launch a new vaccination method, that biology is a completely new science, still in its infancy and full of unknowns, still rich in erroneous knowledge and false dogmas, as evidenced by certain capital errors, proven and corrected in this book.

It must not be forgotten that biological knowledge is not definitive and that that which constitutes the basis of research must first undergo a check that sometimes reserves astonishing surprises.

It should also not be forgotten that bacteriology, currently established on roughly the same basis as it was sixty-five to seventy years ago, has since made no progress in principle or fundamentally and is still governed by the same false dogmas. The proof of the accuracy of these remarks is that, since the discovery of BCG, a new concept of capital importance has been acquired, which demonstrates the serious error of knowledge reputed to be well demonstrated and the profoundly backward state of bacteriology: it is the knowledge of the nature and autogenous origin of Koch's bacillus, an organelle that constructs the tissues of organized living beings, modified in its essential properties by the spontaneous development of the tubercular property in the organism.

This new concept, which completely modified the previously accepted conceptions of tuberculosis and demonstrated its spontaneous development, at the same time destroyed the false dogma of the asepsis of living beings.

The capital importance of these two facts, which was quickly understood, explains to readers the violent attacks to which they were subjected by the conformists, defenders of dogmas.

In the second volume of this work, published in 1936, I provisionally called this constructive organelle, the dumbbell, a mitochondria; it is demonstrated, in this third volume, that mitochondria have no real existence, and are only the formless, scattered debris of the dislocated and destroyed cytoplasmic network.

In reality, the mitochondrion, a formless, isolated, and unconnected element, mobile, endowed with catalytic power, does not exist.

Only the dumbbell organelle exists, of a unique form, universal for all living beings, for all the tissues of the same organism, stable, rigorously immobile in the networks it forms; it is never isolated, and it is always articulated with others by its balls to form networks or filaments (neurofibrils).

The dumbbell organelle, builder of the cytoplasm, is also, with a strictly identical shape and mode of articulation, the organelle that builds nuclear networks, that is, the cell nucleus, and I think that Koch's bacillus or tuberculosis-forming bacillus is the nuclear dumbbell and not the cytoplasmic dumbbell, just as, for cancer, the cancer-forming bacillus, homologous to Koch's tuberculosis-forming bacillus, is also a nuclear dumbbell.

This knowledge now allows us to understand the real significance of **Calmette** and **Guérin's** discovery, which nonetheless remains very important.

* * *

If, at the time of the **Lignères-Calmette** discussions, the nature and origin of Koch's bacillus had been known, as well as the spontaneous development of tuberculosis, which restricts cases of development by contagion to a tiny number, the question of the efficacy and safety of B.C.G would have evolved in a completely different way.

Also, these important notions, which were made known in the second volume of this work in 1936, required a new analysis because of the considerable modifications they bring to the meaning of **Calmette** and **Guérin's** discovery.

In this presentation, various notions, the capital importance of which has not been perceived until now, must be addressed; these are:

1. The nature and principle of immunity, which were not respected in the development of the B.C.G vaccine, the direction of research of which was distorted for this reason;

2. The fact that B.C.G is a bovine bacillus, specifically different from the human bacillus and which, because of this specific difference:

A) Cannot be a vaccine against the latter.

B) Is virulent for humans.

We will examine these two points in turn.

1. THE NATURE AND PRINCIPLE OF IMMUNITY WERE NOT RESPECTED IN THE DEVELOPMENT OF THE B.C.G

What is the nature of immunity, and what does it consist of? The word **immunity** against a disease means: existence, in the immune person, of this disease and of the living virus that causes it. As soon as the virus is dead, immunity ceases. The sole effect of immunity, in the event of a new contamination, is to restrict the bacterial development of the virus in the blood, which generally makes the acute period of the disease benign and short and causes it to quickly pass into the mycelial state, that is, into the state of hyphomycete. The virus of the disease therefore necessarily exists and with all its characteristics in the immune person.

So here is a first, certain, obvious principle. A second principle is that one can only vaccinate against a disease with its virus having retained all its properties, which can only be modified in their intensity, and to a certain extent that must not be exceeded; this is the attenuation of the virus.

The attenuation of the virus is not transmissible by heredity. By successive subcultures of the attenuated virus, by a single change of environment, one modifies its virulence, a fact which signifies that the attenuation is a temporary and not specific modification; this is why the attenuated virus multiplies in the inoculated and vaccinated organism.

In B.C.G, it is quite different: the tuberculizing property of Koch's bacillus, which is essential, specific, appears to be definitively lost, disappeared; by change of environment it does not reappear; by serial inoculation in the guinea pig it does not reappear; the acquired characteristic, the loss of tuberculizing power is therefore hereditarily transmissible with fixity; this loss has therefore become a specific characteristic and B.C.G has become a new species, characterized and different from Koch's bacillus. BCG can therefore no longer vaccinate against Koch's bacillus; it can only vaccinate against B.C.G.

These are the facts that have been lost sight of in the development of B.C.G. It has been forgotten that vaccination against tuberculosis could not be carried out, if it were possible, than by a living tubercular bacillus, that is, a true, attenuated Koch bacillus; but experience has proven that, even by these methods, strong and long-lasting immunity is impossible to obtain.

It is demonstrated in this book that the dumbbell organelle, builder of the organism which, because of this fundamental role, possesses the intrinsic property of multiplying, becomes stable and immobile, that is, it enters a state of rest in the organized elements it has created, as soon as they have become adults: cytoplasmic and nuclear networks of cells, nerve fibers, etc.

This state of rest of the elementary organelle is obviously ensured by a set of conditions of the normal environment in which it lives.

The acquisition of the tubercular or cancer-causing property of the dumbbell organelle consists in the recovery, under the influence of abnormal conditions, of its intrinsic property to multiply and in an exacerbation of this property which is manifested by the power to form embryonic cells, an agglomeration of which constitutes the tubercle and each of which becomes the center of formation of a skein of parallel bacillary filaments (filamentous form of Koch's bacillus) which, by insinuating themselves into the heal thy tissues, constitute the progressive tubercular infiltration.

I have formally demonstrated, in the second volume of this work, the constitution of embryonic cells by the dumbbell organelle (pl. 1 to 20, 2nd vol.); the formation of these skeins of filaments of Koch's bacilli by the embryonic cells (pl. 45 and 49) from which we see them emerge; the formation, by these cells, of isolated Koch's bacilli, bacilli photographed at the very moment of their birth (pl. 45, 46); the formation of filaments of isolated Koch's bacilli, photographed at their birth (pl. 49) and showing that they are born from a large granulation which is, or was, a dumbbell ball of the embryonic cells.

These facts, which are formal and easy to verify, establish in a peremptory and indisputable manner that tuberculization is carried out by the dumbbell organelle, which has become Koch's bacillus, by the embryonic cell, and by the bacillary filaments emitted in parallel bundles by this cell, filaments which are the agents of tuberculous infiltration.

From these facts, it follows that the tuberculizing property lies entirely in the creation of the embryonic cell and that the modification produced by the culture of Koch's bacillus in a bilious medium is the suppression of its property of forming the embryonic cells that constitute the contents of the tubercles.

The B.C.G thus obtained still multiplies like the normal dumbbell, but has lost the exacerbation of the multiplying property that constitutes the deformation or spontaneous degeneration that converted it into Koch's bacillus.

* * *

By losing its tubercular property, what has become of B.C.G? It has apparently returned to its initial state, before acquiring the tubercular property, that is, to the state of a normal dumbbell organelle devoid of this property; at the very least, one can affirm that it has come considerably closer to it.

It is precisely this fact, which is of great importance, that is established by the production of B.C.G by culturing Koch's bacillus in a bilious medium. The fact is formal; by this simple change of medium, the tubercular property disappears.

This fact is of great importance because it establishes something that seemed mysterious and difficult to detect: the cause of the spontaneous appearance of the tubercular property and, by extension, of the cancerous property in the normal dumbbell organelle. Since this property, once acquired, is lost by a simple change of culture medium, it must therefore have appeared by an analogous change which is obviously the suppression of the normal plasmatic medium of the dumbbell and its isolation in an abnormal medium outside the cell, in the pulmonary alveolus for example.

These notions lead to new proof of the vaccinal ineffectiveness of B.C.G. This proof is that the B.C.G having apparently returned to the state of the normal dumbbell organelle

which exists in quantities of several hundred grams in the lung, it is not not a few hundredths or even a few tenths of a milligram of inoculated or ingested B.C.G can exert a vaccinating action that hundreds of grams of the same organelle that already exist normally in the tissues do not exert.

We must therefore conclude:

1. That the new notion establishing that Koch's bacillus is the dumbbell organelle that constructs cells and other elements of the organism, modified in its properties, constitutes formal demonstration of the impossibility for B.C.G, which is this organelle returned to its normal state or to a similar state, to possess a power of vaccination against tuberculosis;

2. That the fact that the Koch bacillus has lost its tubercular power, having become fixed, definitive, and hereditarily transmissible, constitutes a fixed hereditary characteristic for B.C.G, makes it a specifically different element from Koch's bacillus, and prevents it from possessing a vaccinating power against the latter. It can no longer be immunizing except against B.C.G itself, that is, it cannot even immunize cattle against the bovine Koch bacillus from which it originates;

3. That the nature of immunity and its characteristics necessarily imply the notion that a tubercular organelle alone could protect against tuberculosis. The search for a non-tubercular organelle to protect against tuberculosis is therefore based on an error of reasoning.

2. B.C.G IS ORIGINALLY A BOVINE BACILLUS

The bovine bacillus is specifically different from the human Koch bacillus, just as the bovine colibacillus is specifically different from the human colibacillus. Proof of the specific difference between the latter two is that injecting an animal with bovine serum develops in it, through the bovine organic colibacillus it contains, serum sickness, that is, the state of anaphylaxis, a condition created by the multiplication of the bovine colibacillus in the blood.

It follows from this specific difference that B.C.G from a bovine bacillus cannot vaccinate against the human Koch bacillus, which is specifically different in origin. B.C.G could therefore, according to the fundamental principle of immunity, protect only against the bovine bacillus; but we have just seen that, through the definitive and hereditary loss of tubercular power, B.C.G has lost, by that very fact, all possibility of protective action, even against the bovine bacillus.

Whatever the aspect of the problem, we therefore arrive, in all cases, at the conclusion of the impossibility of a protective action of B.C.G against tuberculosis.

The facts that dominate the question, those that carry the definitive decision are:

1. The nature and origin of Koch's bacillus, the elementary organelle that builds the organization of tissues, modified in its properties;

2. The fact that the tuberculizing property of Koch's bacillus consists of the exacerbation of the intrinsic property of dumbbell multiplication, a property kept dormant in the organization of tissues;

3. The fact that B.C.G is a tuberculizing Koch's bacillus that has become a non-tuberculizing dumbbell organelle;

4. The fact that a bovine bacillus, specifically different from the human bacillus, cannot vaccinate against human tuberculosis;

5. The crucial fact that the normal lung contains a large quantity, hundreds of grams, of normal, non-tuberculizing dumbbell organelles like that of B.C.G and that it is not the inoculation of 1/10 of a milligram of the latter that can have the protective action that hundreds of grams of normal dumbbell organelles do not exert;

6. The crucial fact that, despite the presence of this large quantity of normal, non-tuberculous dumbbells constituting the organization of the tissues and similar to those of B.C.G, tuberculosis develops spontaneously in the organism by the modification of the properties or the spontaneous degeneration of these same normal dumbbells.

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We now have to examine whether the facts, and in particular the experimental results, are in agreement with the conclusions just presented.

1. THE EXPERIMENTAL RESULTS INDICATE THAT THE INNOCULATION OF B.C.G IN CALVES CONFERS ON THEM A CERTAIN DEGREE OF PROTECTION.

This is the most favorable result that has been obtained with B.C.G. Is this really a true immunization, while **Lignères** specifies on this subject: that we must reject the completely erroneous idea of a very strong and long-lasting vaccination; then, further on that: In any case, we must not lose sight of the fact that vaccinated subjects all become tubercular if they are subjected, after a certain time, sometimes quite short, to somewhat severe injections.

Let us emphasize here that these results apply to bovids and are not applicable to humans.

It was such results obtained on bovids that prompted Messrs. Calmette and Guérin to apply the BCG vaccine to humans.

I recall here **Lignères'** conclusion on this subject: It must be remembered that the CALMETTE and GUERIN vaccine can only give relative and temporary resistance that weakens and disappears fairly quickly.

Such results cannot be considered as proving the achievement of true immunization. The fact that B.C.G is a dumbbell organelle which, having lost its tuberculizing property, no longer appears to differ significantly from the normal dumbbell organelle, makes it difficult to understand why it would possess the immunizing power against Koch's bacillus that it did before its culture in a bilious medium, since the organism of bovids contains an enormous quantity, in kilograms, of non-tuberculizing dumbbell organelles, similar to those of B.C.G and which have no immunizing effect.

In any case, these results are clearly insufficient to justify the application of such vaccination to humans.

Let us also recall the results of Watson's experiments in Canada, who observed that, when hundreds of guinea pigs were inoculated with B.C.G, a certain number of them developed, after a year at most, tuberculous lesions that could be serially reinoculated. This observation by **Watson** requires great caution.

2. RESULTS OF VACCINATIONS PRACTICED ON HUMANS

I will simply refer the reader to **Lignières's** criticism of these results, a criticism that I find rigorously fair and measured. As for the statistics that were produced to justify these results, I would like to point out that the criticisms they have given rise to from the most qualified specialists in this field have made it clear that the serious errors they contain deprive them of any documentary value.

We know that until now, one of the ways to assess whether B.C.G creates a certain degree of premunition has been to determine, by skin reaction, the state of allergy that could result from its inoculation or injection. I will not discuss here the results observed by this process and which can be found in **Lignères'** book.

I will simply point out that skin reaction statistics, carried out on men aged 18 to 20 (army recruits) deemed healthy after a careful medical examination, indicated positive reactions in 90, 95, and even up to 98% of subjects.

It is obvious that if such subjects were all tubercular, the human species would have ceased to exist long ago.

Now, the new knowledge of the nature of Koch's bacillus, a tissue-building organelle modified in its properties, and the knowledge of the presence of the normal dumbbell organelle in all the body's tissues, provides a new element of capital importance in the appreciation of the significance of the positive skin reaction in subjects who do not have tuberculous lesions and who subsequently remain free of tuberculosis.

Moreover, I repeat once again that the B.C.G. dumbbell organelle, brought back by culture in a bile medium to the state of an apparently normal dumbbell organelle, cannot have an immunizing action and that it can have one even less since it is the dumbbell organelle of beef, specifically different from the human dumbbell organelle, it can, for this reason, only exert a virulent action and no immunizing action.

In short, this application of vaccination to humans has not provided proof of a preventive and useful action of B.C.G.

What can be said, on the other hand, of **Calmette's** assertion of a considerable reduction in mortality from diseases other than tuberculosis in children vaccinated with B.C.G and the production of statistics in which the general mortality rate would have been 18.5% in 27 unvaccinated people (5 deaths) while it would have been only 2.3% in 210 vaccinated people (5 deaths), that is to say, 8 times lower **(1)**.

Such exaggerated, unbalanced, and unpredictable comparisons are all the less likely to reinforce the belief in the protective action of B.C.G against tuberculosis, since **Calmette** does not say and we do not see where this protection against non-tuberculous diseases could come from, especially now that the nature and origin of Koch's bacillus and those of B.C.G are known, as well as the presence in the body of the normal dumbbell organelle, apparently similar to B.C.G and which should, it seems, by the kilos of this organelle that the body contains, exert a much more favorable action than the hundredths of a milligram of B.C.G that is inoculated into children.

We will see later, on the other hand, that being specifically different from the normal human dumbbell, the ox's B.C.G dumbbell can only be harmful and virulent for him, and cannot, consequently, exert a favorable effect on his organism.

* * *

One question remains to be examined: that of the harmlessness or virulence of B.C.G. The accidents that have occurred in all countries and which have been published now make it impossible to dispute the virulence of this vaccine.

(1) *Calmette: Practice and currently known results of preventive vaccination of tuberculosis in early childhood by tuberculosis" (Bull. of the Ac. de Méd., meeting of November 6, 1928)*

The serious cases of Doctor **F...** and the children Denise and Marie D..., vaccinated with B.C.G, establish this virulence in a decisive, irrefutable manner, as **Lignères** has proven.

Moreover, the proof of this virulence has been well noted and accepted, namely that the doses of B.C.G injected under the skin, which initially were 5 to 10 milligrams, subsequently fell to 1/100 or half a hundredth of a milligram.

B.C.G is therefore not harmless. The proportion of accidents is generally low, especially that of serious accidents, but the existence of virulence is certain and indisputable. The following will confirm this.

Various communications from the Health Committee of the League of Nations concluded in 1928:

1. That vaccination with B.C.G according to the **Calmette-Guérin** technique proves to be perfectly harmless in animals of the bovine species;

2. That B.C.G administered orally to newborns in the first ten days of life and subcutaneously to older children and adults proves incapable of causing virulent tuberculous lesions.

These Commissions remained silent on the safety of this vaccination in humans.

Regarding these conclusions, **Lignères** had objected (42, p. 170) that the safety of B.C.G in bovines did not allow one to conclude that it was safe in newborns, since, in certain cases, they have been observed to have a harmful, sometimes serious, reaction to these vaccines.

At the time of these discussions, the facts revealed in 1936 in the second volume of this work on the nature and origin of Koch's bacillus were unknown, notably that it is the halter organelle that builds the body's tissues and that, through degeneration, it has become tubercular and that, through culture in a bilious medium, it becomes an apparently normal and non-tubercular halter organelle again.

However, B.C.G is a bovine bacillus, that is, the bovine halter organelle, and therefore specifically different from the human halter organelle. This specific difference implies that the bovine halter organelle inoculated into cattle must not or may not be virulent for them, but that, inoculated into humans, it will be harmful, or at least could cause disorders in their bodies. Here is the proof:

The injection of human blood or serum into humans does not cause any disorders. The injection of bovine serum into humans will cause serum sickness with urticaria and the

state of anaphylaxis with the development in the blood of the proteolytic ferment discovered by **Abderhalden.**

This difference in action is explained by the following fact:

The state of anaphylaxis is created by the elements of the normal colibacillus of the antigenic serum (see colibacillary function); its elements exist in the blood of all higher animals, both in human blood and in bovine blood. Human colibacillus injected into human blood causes no disorders there. Bovine colibacillus, inoculated into humans, causes serum sickness and the state of anaphylaxis.

Thus, the normal elements of the organism of an animal species, injected into the same species, do not cause any problems; injected into a different species, they cause infection and a state of anaphylaxis, even though they are morphologically similar and possess substantially the same properties; it is the specific difference of origin that acts here.

Thus, if the inoculation of the ox with colibacillus, a normal element of its organism, is completely harmless to the ox, it is, on the other hand, harmful to humans and causes serum sickness in them.

Likewise the inoculation of another normal constituent of the organism, the dumbbell organelle of the ox, may be indifferent to the ox and will be harmful to man. The B. C. G. which is this organelle of the ox which has become tuberculizing by degeneration, then returned to the state of a non-tuberculizing organelle by culture in a bile medium, cannot therefore be less harmful but rather more, for humans, than the normal dumbbell organelle of the ox.

I repeat once again that, the pasteurian dogmas having arrested the evolution of bacteriology and a certain part of the biological sciences, there exists a large quantity of unknown facts of capital importance which can radically modify certain notions currently accepted in science. This book provides striking examples with the false notion of mitochondria, the existence of the dumbbell organelle, the coli-bacillary function, the false dogma of the asepsis of living organisms, the initial state of viruses and their original source... etc.

Consequently, extreme caution must be exercised when applying a vaccination process to humans. Introducing living microorganisms, even attenuated ones, into the human body is something that can have, in the long term, unexpected effects. Before proceeding to practice, one must be completely informed, which was not the case for the B. C. G. since we proceeded to inoculations without knowing what this microorganism was, nor where it came from, knowledge which it was however possible to acquire since I explained it in the second volume of this work.

I wrote, at the beginning of this chapter, that current biology was still full of unknowns. The reader will note, in this regard, that vaccination with B.C.G against tuberculosis was put into practice despite the following four key unknown concepts:

1. Ignorance of the existence of the dumbbell organelle, constructor of animal and plant tissues;

2. Ignorance of the nature and origin of the Koch bacillus, a dumbbell organelle which has become tuberculous through degeneration;

3. Ignorance of the specific difference that exists between normal human and bovine dumbbell organelles, and between human and bovine tuberculizing dumbbell organelles, a difference which makes immunization of one against the other impossible and explains the harmfulness of inoculation of the normal dumbbell organelle from one animal species to another;

4. Ignorance of the passage of heterogeneous viruses from the bacterial state to the mycelial state in the animal organism and of the fact that this passage constitutes the state of immunity, but at the same time constitutes a chronic phase of the disease caused by these viruses. From which it follows that immunization is impossible against a virus which does not determine a bacterial phase, and invades the body immediately in the mycelial form.

The reader will judge whether, under these conditions, the implementation of B.C.G vaccination was and still is very premature, and whether it is even tolerable for this vaccination to still be practiced.

I also recall that, with regard to the use of anti-diphtheria serum and vaccination with diphtheria toxoid, these procedures have been put into practice, despite very serious shortcomings in the preparation of the products used in these procedures. These shortcomings result from ignorance of facts of capital importance and, first and foremost, from a total absence of sufficient study and determinism concerning the nature and origin of viruses in general and of diphtheria and tetanus viruses in particular.

GENERAL CONCLUSIONS

1. Koch's bacillus is the dumbbell organelle and, particularly, the nuclear dumbbell organelle of cells of the organism in the process of destruction;

2. Just as the human organic colibacillus is specifically different from the bovine colibacillus, the latter being virulent for humans and causing serum sickness and anaphylaxis, the human dumbbell organelle is specifically different from the bovine dumbbell organelle and the latter, like the bovine colibacillus, can only be virulent for humans;

3. Because of the specific difference that separates the human and bovine dumbbell organelles, the human Koch bacillus cannot vaccinate against the bovine bacillus and, reciprocally, the latter against the human bacillus;

4. B.C.G is the tuberculizing bovine dumbbell organelle deprived of this tuberculizing power by culture in a bilious medium and thus returned to the type of the normal dumbbell organelle;

5. The loss of the tuberculizing power that characterizes B.C.G, which is stable and maintained by serial inoculations in animals and which, consequently, has returned it to the state of a normal dumbbell organelle, specifically differentiates it from the bovine Koch bacillus and deprives it of the immunizing power against it, that is, against bovine tuberculosis;

6. Due to the specific difference between the normal bovine and human dumbbell organelles and the difference between the bovine and human Koch bacilli, B.C.G cannot possess any immunizing power against human tuberculosis;

7. Since the bovine dumbbell organelle cannot be, like the bovine colibacillus, virulent for humans, B.C.G also possesses the same virulence;

8. Diseases caused by heterogeneous viruses are dangerous in their acute phase due to the fermentative action of their micrococcal elements. These diseases immunize by the passage of the virus from the bacterial state to the mycelial state, which constitutes their chronic phase, characterized by the disorders caused by the vegetation of the mycelium in the organs; immunization therefore consists of conferring on the vaccinated person a chronic disease which is not exempt from progressive development and disorders, but which, in the event of a new infection, will rapidly cause the bacterial virus to pass into the mycelial state;

9. Vaccination can only be provided against the bacterial phase of heterogeneous viral diseases, not against the mycelial and chronic phase, which is inevitable. Immunization against diseases with mycelial and chronic development from the outset (or with excessive vegetation of the dumbbell organelle) such as syphilis, tuberculosis, and cancer is impossible.

CHAPTER XIV

STUDY OF THE CAUSE OF CANCER AND ITS MODE OF DEVELOPMENT

In his *Outline of Pathological Anatomy*, **Letulle** (**Masson**, 1912) indicates that the development of neoplastic tissues is governed by the following three essential principles:

1. Every tumor has, in the body, its normal homologous tissue, either in the adult or embryonic state, a principle due to **J. Muller** (1838);

2. Every tumor is the product, by direct filiation, of the multiplication of pre-existing cells, a principle due to **Virchow** (1850);

3. All tumor tissue is marked by a specific familial heredity, connective or epitelial, which it cannot escape; this principle, due to **Bard**, dates from 1890.

This means that the evolution of cancer consists of the neoformation of tissues similar to those that exist. This neoformation is obviously due to a special cause, which some have considered inherent in the neoformed tissue itself and, others, to the presence of heterogeneous organisms parasitizing normal elements and modifying their properties.

Many authors have described various bacterial species as specific agents of tumors; among them, we note **Scheurlen's** bacillus, **Manfredi's** diplococcus, **Delbet's** lymphadenoma bacillus, and **Doyen's** micrococcus neoformans. **Letulle** wrote about the latter:

Doyen succeeded in staining, inside the protoplasm of tumor cells, in extracting and cultivating a micrococcus, Micrococcus neoformans, which this author believes to be the pathogenic germ of all tumors, both benign and malignant. With the help of cultures of this micrococcus, one could reproduce, at random in experimental series, in particular on rodents, the complete series of neoplasms common to humans and animals. This microbe, which closely resembles the polymorphic coccus of the skin, a common saprophyte, undoubtedly causes experimental inflammatory lesions, hyperplastic, very varied, up to and including polypoid, even adenomatous productions; but the indisputable proof of its role in the genesis of cancer has not yet been established, in our eyes, on an indisputable basis.

Let us note after this quotation that the new knowledge of the two elementary organelles, formative of the animal organism, the constructive dumbbell and the micrococcal granulation of the colibacillus, allows us to say:

1. That the colored intracellular granulations observed by **Doyen** are certainly the balls of the cytoplasmic dumbbells and not free granulations;

2. That the micrococcus that he isolated in culture is not this immobile intracellular granulation, but the free granulation of the blood, the microzyma of **Béchamp** (old molecular granulation), that is to say the micrococcal form of the colibacillus, that which constitutes the fibrinferment and which swarms in tumors, a fact which explains its resemblance to the polymorphic coccus of the skin which is also the colibacillary micrococcus.

* * *

Other authors have considered certain forms of fungi that they have observed in tumors to be specific agents of cancer. I recall here that I myself observed the presence in a breast cancer of the hyphomycete which is, with absolute certainty, the real syphilis virus, of which **Schaudinn's** treponema is only a spiral appendage of the mycelium.

But this fact did not lead me to conclude that this hyphomycete was the cause of the cancer, because it was certainly present in the breast as everywhere else in the subject's body and that, consequently, its presence is not proof of a specific action in cancer.

One of the latest purported cancer-specific agents described among fungi in recent years is Mucor neoformans of **Enderlein**, a species of mucor which he considers to be the "culminating" developmental form of Micrococcus neoformans of **Doyen**, a form which he indicates to be identical in every respect to Rhyzopus nigricans **Ehremb**(1818).

This conclusion, like the one that attributes tuberculosis to the development of an *Aspergillus* in the organism, is an error of **Enderlein,** coming from the fact that he took for a phase of the development of the hyphomycetes the element that he called **chondrit**, a rod ending in a ball and that he did not realize that this form is only a part of the universal constructive organelle of all living beings, animals and plants, the dumbbell. He saw this **chondrit** develop inside and outside the filaments and spores of molds and, as he also found it in the blood and on the blood corpuscles of cancer patients, and even on those of healthy individuals, he concluded that all individuals are parasitized by this **chondrit** which, usually, does not cause disorders and which, only under certain conditions, notably by a decrease in pH, would become pathogenic.

These facts were published in a memoir by **G. Enderlein** in 1937 (18 D) entitled: "*Study on the problem of cancer. The evolutionary cycle of the cancer agent: Mucor neoformans*, **Doyen** 1902."

The new notions presented in this third volume on the anatomical constitution of the cytoplasm and the nucleus of the cell demonstrate that **G. Enderlein's** chondritis, presented as the parasite that causes cancer, is only a part of the normal dumbbell orga-

nelle, the universal constructor of normal cells and tissues of living beings, animals and plants. It is this organelle in the process of vegetation that he observed, but of which he did not see the exact dumbbell shape, nor the articulation by its balls, nor the constructive role.

It is this dumbbell whose vegetation I showed in the second volume, forming the embryonic cells of tuberculous tissue, consisting exclusively of dumbbells which, in turn, give rise to Koch's bacillus and Koch's bacillus filaments. This is so simple and clear that we are witnessing, so to speak, the birth of Koch's bacillus in the plates of the second volume.

The demonstration, in this third volume, of the formation of the cytoplasmic network by the universal dumbbell organelle, and of the formation of the nuclear network by this same dumbbell, completes the demonstration that it is normal for tuberculous tissue, like all living tissue, to be constituted by this universal dumbbell which, in this particular case, through its degeneration, has become Koch's bacillus. One must be blind and an unrepentant conformist to dispute such a truth.

It is the false and harmful Pasteurian dogma of the asepsis of living organisms that led bacteriologists down the wrong path and forced them to consider Koch's bacillus as a heterogeneous microbe.

It is therefore exclusively the normal elementary organelle dumbbell, the one that constitutes our normal cells and tissues, that **G. Enderlein** observed, but whose exact form he did not see or even suspect the universal constructive function.

On the other hand, this dumbbell does indeed become the organelle that constructs cancerous tissue, that is, the cancerous virus, when, through degeneration, it has lost the stability it exhibits in normal tissues. Through its exuberant, unlimited vegetation, it then becomes, like the tubercular dumbbell, a true parasite that destroys normal cells and invades the tissues it gradually destroys.

In the origin of cancer, in the nature of the virus, in its mode of spread, there is therefore an absolute identity between cancer and tuberculosis. In both cases, the virus is the degenerated dumbbell organelle. Both surely have, as their primary origin, a nutritional disorder of the cellular dumbbell and, in particular, of the nuclear dumbbell.

* * *

The following study aimed to:

1. Determine which elements, by their multiplication, cause the spread of the cancerous mass.

2. Determine the nature and form of the elements which, by being transported far away, cause infection of the lymph nodes and metatases;

3. Determine the mode of propagation of these elements.

The following presentation concerns three different varieties of cancer that we will study successively:

1. On extremely rapidly growing buds of a recurrence in the surgical wound and three weeks after surgery, of a uterine epithelioma;

2. On a glandular cancer of the breast with considerable connective tissue proliferation;

3. On two globocellular sarcomas of the breast.

I. Histological study of the cancerous buds of a rapid recurrence on the surgical wound of a uterine epithelioma

In the preparations (sections of a bud fixed with 10% formalin, stained with ferric hematoxylene, plate 44), we notice: A) Large areas of tissue containing neither cells nor connective elements.

There are only a multitude of dumbbells articulated together by their balls and forming a tissue similar to that of the cytoplasm of the cells of the spinal ganglia (see figs. 1, 2, 3, pl. 16). The appearance of this tissue, which differs completely from that of the cytoplasm of the cells of the glandular tissues, shows that the assembly of the dumbbells, which, however, always constitutes networks, occurs without any order, that is to say, without any tendency to form either a network or cellular elements. These areas demonstrate the extreme activity of the multiplication of the dumbbells at the same time as its incoherence. A large area of this kind is found in the middle of figure 1, plate 44 (magnification 220), and in figure 2 at magnification 1100.

B) In other regions, areas of cells are observed whose nucleus is formed by a bundle of dumbbell filaments (fig. 3, 4, pl. 44). In figure 4, pl. 45, we see in C. D. 6, a nucleus for which this origin cannot be doubted. In figure 6 of plate 45, we see a bundle of fibers F. 6 whose ramifications give rise to several nuclei.

These bundles forming nuclei or cells are very clearly seen in figures 3 and 4 of plate 45. I also recall that, in the second volume of this work, I have already pointed out the mode of formation of the epithelial velules (a plate 17) of the alveolar wall of the human lung by a formative branch (b plate 17) constituted by dumbbell filaments.

In the same vein, I will recall that this plate 17 of the second volume also shows:

1. The emission of dumbbell filaments (n) by the granulations of the cells (m);

2. The formation, by these emitted filaments, of embryonic cells (to the left of cell A).

All these formations are the consequence of the multiplication property acquired by the dumbbell organelle when the normal conditions of its nutrition are modified. The formation of nuclei or cells by a bundle of dumbbell organelle filaments has its equivalent in the formation of the Früchtkörper in Hyphomycetes by spiral winding of a mycelial filament.

C) There are other areas of cells in which only the nucleus is generally distinguishable, the cytoplasm being altered or in the process of caseification. The nucleus is made up of a considerable number of dumbbells, one ball of which is articulated with others to form a large karyosome, the other ball being articulated either with the wall of the nucleus or with other balls in another karyosome. This arrangement results in very varied figures that some have wrongly confused with karyokinesis figures.

In the extremely rapidly developing cancerous bud studied here, images of actual karyokinesis are extremely rare. Only one that appears to be a prophase stage is seen in the lower right corner of Figure 3, Plate 45.

The type of constitution of these nuclei is that seen in No. 7.8, Figure 2, Plate 45.

This nucleus is drawn, as accurately as possible, in Figure 29 of Plate 40, by varying the focus of the preparation. In summary, the entire formation of the cancerous bud in this case of uterine epithelioma is due to an intense multiplication activity of the dumbbell organelle.

The formation of the cells is due either to the development of embryonic cells formed by a dumbbell, or to the formation of cells and nuclei by a bundle of dumbbell filaments. The phenomena of karyokinesis played no role in this multiplication.

II. Conjunctivo-epithelial breast cancer

The study of this very interesting case was made on sections of a tumor fragment fixed by **Bouin's** fluid in a Parisian hospital surgery department; the fixation altered the cells, but respected the connective tissue. The most important fact observed in the study of these sections relates to the nature and origin of the connective fibers which form the vortices.

These connective fibers are dumbbell filaments, emitted in compact bundles by embryonic cells (figs. 1, 2, 3, 4, pl. 46). The dumbbell filaments are not very visible in the obliquely cut bundles. They are quite clearly visible in the bundles located at the upper left and right corners of figure 2, and very clearly visible in figure 4, in I. 7, and in M. 7, plate 46. The emission of these dumbbell fiber filaments is seen in figure 2 in I. 11, in figure 3 by embryonic cell E. F. 3 and in figure 4 by embryonic cells F. 13-14 and by the group of embryonic cells located in the upper half of the left edge of the same figure.

Thus, the formation of connective fiber bundles in cancer and tuberculosis is revealed to be strictly identical in nature and origin. These fibers are dumbbell filaments emitted by embryonic cells in both cases. These dumbbells are tubercle bacilli (Koch's bacillus) in the case of tuberculosis. They are therefore cancerous bacilli in the case of cancer, and it should be possible to cultivate the cancerous bacillus just as one cultivates Koch's bacillus.

Figures 1 and 2 of Plate 47 show islands of glandular cells more or less altered by the fixative; for example, a group of embryonic cells of various sizes equipped with their dumbbell or formative pedicle has formed in K. L. M. 5, Figure 2, Plate 47, in place of disorganized cells. Others are formed at other points in Figures 1, 2. A cell altered by the fixative and displaced F. 7, 8, is still attached to its formative pedicle D. 8.

III. Globocellular Sarcoma of the Breast No. 1.

The study of this case of cancer has revealed the following new facts:

1. The nature of the globocellular elements of this case of cancer, elements which are exclusively nuclear masses, not surrounded by cytoplasm. They are therefore not true cells;

2. The constitution of these elements, exclusively by dumbbell organelles, clearly distinguishes them from leukocytes or lymphatic cells constituted by simple granules; one could confuse them with the latter because they are seen exclusively inside the lymphatic ducts, at least at the beginning of the infiltration or spread of the cancer;

3. The vegetation or germination of these masses by giving rise to multiple and long filaments which are the agents of cancerous infiltration. These filaments enter the lymphatic ducts and form new globocellular masses which will constitute new foci of spread;

4. The new globocellular elements are first formed in the state of small embryonic cells by a ball of a dumbbell;

5. The cancerization of the lymph nodes occurs in the same form of globocellular elements as the initial cancer;

6. This nuclear form of the cancerous elements seems to indicate that the starting point of the lesion is a cell nucleus or a group of cell nuclei whose nutritional conditions have been greatly altered, a fact that has led to the degeneration of the dumbbells that constitute them.

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Let us now examine the preparations that have made it possible to establish these facts. It is always through the lymphatic channels that the cancerous infiltration occurs, spreading to the lymph nodes. Drawings of microscopic preparations of cancer never show the lymphatic channels; microscopic photographs sometimes do.

Histology treatises indicate that the lymphatic channel system is originally formed by stellate cells whose branches abut and fuse together to form a reticular system, but they do not specify how the branches of these stellate cells empty their contents to form a reticular system of continuous channels that connect with each other.

This is the reticular system seen at the periphery of cancerous tissues, in the infiltration zone; it is seen at a magnification of 110 in Figures 1, Plate 47; 1 and 3, Plate 48, and at a higher magnification in Plates 49 and 51a.

Here, the interior of the areolas appears to contain no elements.

In reality, the periphery of the tumor was made up of fatty tissue of which the reticular system of lymphatic canals was the framework. This reticulum was emptied of its fat by its prolonged stay in alcohol and toluene for inclusion in paraffin.

The fact that large nuclei such as J. 11, figures 3; I. 6, figure 4; B. 2, figure 1, plate 49, exist in lymphatic canals with a diameter five or six times smaller than their own demonstrates that these masses developed on the spot from a granulation of a dumbbell filament which traveled far into the canals, then considerably enlarged.

Examination of the periphery of the tumor shows that there are pathways through which cancerous infiltration occurs more rapidly than elsewhere. Figure 3, Plate 48, shows one of these paths on the left side. Figure 4, Plate 48 shows another at a higher magnification. The very thin wall of normal lymphatic ducts is almost transparent (Fig. 1, Plate 51a). As soon as the cancerous dumbbell filaments have penetrated the ducts, they become stained dark black with ferric hematoxylin. The path of the cancerous material is thus very easily detected in the preparations.

Like the embryonic cells of tuberculous tissue, the globular masses of the sarcoma studied here are formed by a dumbbell, one ball of which forms a dumbbell filament that curls up. Once adult, the mass germinates and gives rise to a large number of long filaments, like the tuberculous embryonic cell, whose emitted filaments group into a bundle. In the cancerous tumor, on the contrary, they move in all directions.

We will see the formation of numerous young embryonic cells in an infected lymph node in the armpit of the patient with the tumor; we see a group in region F. II, figure 1, plate 50; we see others at numerous points in figures 1, 2, 3, 5, plate 50.

As for the germination of adult masses to form multiple filaments, various examples are seen in plate 51: in E. 3, 4; in E. 7, 8, figure 1; in H. 9, figure 2; in G. 7, figure 3; in J. 5, 7, 8, figure 5. These filaments are directed in all directions and become entangled as shown in figure 3, plate 51. They cross the wall of the lymphatic duct and destroy it completely.

In summary, the multiplication and spread of cancerous tissue occurs in exactly the same way as that of tuberculous tissue: by the initial formation of embryonic cells which grow, germinate, and emit a large number of filaments which are the agents of cancerous infiltration; by entering the lymphatic channels far away, then beyond, they determine new foci (metastases).

IV. Globocellular Sarcoma of the Breast No. 2.

This case of breast sarcoma is, histologically, exactly similar to the previous one. It is represented by the four figures on Plate 51 bis and by Figure 4 on Plate 50.

These are sections stained with ferric hematoxylin. Figure 1 of plate 51 bis shows the lymphatic network far from the tumor, Figure 2, this same network in the infiltration zone. Figure 3 shows a ruptured lymphatic duct from which many young embryonic cells are escaping, filling it. In Figure 4, which is located in the middle of the already infiltrated zone, we see in the upper right corner a group of young and adult embryonic cells in the process of emitting filaments and, scattered throughout the figure, numerous embryonic cells of all sizes and in the process of development. The lymphatic ducts are in the process of destruction.

These embryonic cells, devoid of cytoplasm, are exactly similar to those of globocellular sarcoma No. 1. They are composed entirely of dumbbells; the rods of these are not very apparent because they are quite strongly dechromatinized by the fixative. However, a certain number of them can be distinguished. Moreover, one can even see, in one of these masses of dumbbells, a karyosome (in R. 14, 15, fig. 5, pl. 50) which owes its star-

like appearance to the radiating rods of the dumbbells that constitute it. This karyosome, by itself, constitutes proof that these large adult embryonic cells, masses called globocells, are, without any possible doubt, nuclei and that the dumbbells that constitute them are of nuclear origin.

We have already seen in the study of sarcoma No. 1 that these elements, although contained at a certain time in the lymphatic ducts, have nothing in common with leukocytes, the latter being constituted by simple granules which are elements of the organic colibacillus.

It follows from these observations that cancer is a disease of the dumbbell organelle and not a disease of the organic colibacillus.

CONCLUSIONS

The study of these four cases of cancer, supplemented by that of numerous other cases that I have not deemed useful to report here, has brought to light the following notions:

1. The active agent of cancer is the nuclear dumbbell of certain cells of the organism;

2. This nuclear dumbbell becomes virulent through a modification of its properties or degeneration due to a change in the normal nutritional conditions of the cell and its nucleus. The trauma, which has considerable etiological importance for breast cancer, probably acts by causing the rupture or obliteration of certain vessels, by dislocating the organization of certain regions and thus depriving cellular elements of their normal plasma irrigation; 3. The dumbbell thus degenerated differs from the normal dumbbell by a property identical to that of the tubercular dumbbell: the considerable exaggeration of its power of multiplication, which is at rest in the normal cell and in its nucleus.

The dumbbell thus becoming cancerous is the cancerous bacillus, just as the Koch bacillus dumbbell is the tubercle bacillus. It must be possible to cultivate the cancerous bacillus in vitro as well as the tubercle bacillus;

4. The cancerous dumbbell acts, like the tuberculous dumbbell, by forming young embryonic cells which develop and grow;

5. Cancerous embryonic cells evolve exactly like tuberculous embryonic cells by germina ting and giving rise, through their dumbbells, to numerous filaments which are the agents of cancerous or tuberculous infiltration. Insinuating themselves far into healthy tissues, they develop new embryonic cells there;

6. The bundles of connective fibers which constitute the horny vortices and globes are bundles of dumbbell filaments formed by embryonic cells;

7. The filaments formed by embryonic cells (or globocellular elements) enter the lymphatic ducts where they develop new embryonic cells which will become new foci of filament emission. It is by this process that cancerous infiltration takes place which, as we have known for a long time, takes place via the lymphatic vessels;

8. The globocellular masses, at least those of the globocellular sarcomas that I have studied, are made up of dumbbell organelles and have nothing in common with leukocytes, which are made up exclusively of simple granules of the organic colibacillus. The fact that, in the area of cancerous infiltration, these masses are contained in the lymphatic ducts has no relation to their nature and origin;

9. The multiplying activity of the dumbbell is exerted in certain cancers by reproducing complete cells, probably similar to the original necrotic cells that gave rise to the degenerated cancerous dumbbell. But this activity can also manifest itself by the formation of masses of tissue composed exclusively of dumbbells articulated by their balls and having no tendency to form cells;

10. There is complete identity between cancer and tuberculosis, both caused by the degenerated dumbbell organelle, evolving in an identical manner in both cases. The difference that appears to exist between them lies in the original source of the degenerated dumbbell, a dumbbell from the nucleus of a noble cell of the mammary gland evidently having very different properties from those of a nuclear dumbbell from an epithelial cell of the pulmonary alveoli.

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CHAPTER XV

STUDY OF RABIES

CAN THE EXALTED RABBIT VIRUS PROTECT MAN AGAINST THE DEVELOPMENT OF RABIES IF INJECTED TWO OR MORE DAYS AFTER INOCULATION WITH THE STREET RABIES VIRUS BY THE BITING DOG?

The influence that **Pasteur's** work has had on the evolution of biological and medical sciences is now in the realm of history. The publication of this work is far enough removed from our time (about three-quarters of a century) for it to be judged solely in the light of pure scientific criticism and in all fairness. This is what is done in this work.

Studies on the experimental creation of immunity had already taught in 1885 that, to obtain it without risks, it is necessary to attenuate the virus of the disease against which one wishes to protect a new animal.

For pasteurian anti-rabies vaccination, it was quite different. Here, it was not a new subject that was involved, but a subject bitten by a rabid dog, that is, one that could already have been infected by the rabies virus.

Contrary to everything that experimental medical studies had established at that time on infection by viruses and the creation of immunity, **Pasteur** claimed to protect against rabies a man bitten by a rabid dog, that is, one presumed to be infected by the rabies virus, by injecting him with repeated doses of this same virus, the virulence of which had been considerably increased by successive passages through the rabbit's body.

He began by injecting marrow in which the virus had been attenuated to a certain extent by desiccation, but after the ninth day, he injected, for three consecutive days, marrow that had retained the virulence of the fixed virus.

A man inoculated with the street virus contracts, like the dog that bit him, a rabies with furious, violent, and convulsive attacks. If the virus of this rabies is successively inoculated into a series of rabbits, the latter contract a disease of a completely different form, which is paralytic rabies, also called experimental rabies and laboratory rabies; moreover, the incubation period gradually shortens, which means that the virus becomes more active.

Thus, while the incubation period of dog rabies, street rabies, varies from 20 days to 4, 5, or 6 months, paralytic rabies in rabbits has an incubation period that, through successive passages, gradually reduces to a minimum of 6 or 7 days; this is **Pasteur's** fixed

virus. These data being known, here is the experiment that served as the basis for anti-rabies treatment:

Pasteur inoculates a rabbit each day with his fixed virus intracerebrally; the rabbit dies around the 7th day. Its marrow is then extracted and dried.

This is done for the rabbit inoculated the following day and then for all the rabbits inoculated up to the 12th or 14th day. The virulence of the marrows gradually decreases with the duration of desiccation. It is therefore strongest for the last three inoculated (no. 432), weakest for the first, no. 12.

At the beginning of the application of the method, **Pasteur** performed a preventive inoculation on each subject every day, starting with marrow number 12, which is the least virulent. To do this, a fragment of marrow is crushed and emulsified in a sterile broth. The first vaccinations did not give rise to any incidents at first. Also, the daily press releases extolling the merits of the method, which saved the bitten from certain death, provoked public enthusiasm. But the results were not satisfactory.

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Cases of rabies soon began to break out among the vaccinated, thus proving the ineffectiveness of the method; they had a moral effect that was all the more disastrous because scholarly and intense propaganda had announced infallible protection and prepared the entire world for the announcement of the sensational results that were about to be obtained.

Following this failure, **Pasteur** modified his method and made it intensive. This time, he carried out three inoculations per day, inoculating marrows 12, 11, and 10 on the first day, then 9, 8, and 7 on the second day, and 6, 5, and 4 on the third.

He then carried out one inoculation per day with marrows 3, then 2, and finally 1, and continued with a daily injection with marrows 4, 3, 2, and 1. Depending on the case, he repeated this last series once or twice more.

The results of this intensive method soon proved catastrophic; reliable statistics including the name and address of each person indicate that: during the year 1886, there were 17 deaths among unvaccinated bite victims and 18 deaths among those bitten who had undergone **Pasteur** treatment; there were therefore 35 deaths from rabies in 1886, whereas the annual average was only 29 or 30 previously.

It was thus formally proven that **Pasteur's** treatment had no effect in preventing the development of rabies in those bitten. But, in addition, another extremely serious observation had been made in 11 of the deceased bitten victims; they had not died with the symptoms of furious dog rabies, they had died with the characteristic symptoms of

rabbit rabies, paralytic rabies, experimental or laboratory rabies, which led several doctors to conclude that these 11 individuals had not died of rabies transmitted by the bitten dog, but of paralytic rabies transmitted to the subject by the injection of the fixed rabies virus contained in the spinal cords of rabbits inoculated by the new intensive method. Indeed, it was from the moment this intensive method was applied that cases of death from paralytic rabies were observed.

In these eleven cases, therefore, injections of rabbit marrow containing the overactivated fixed virus, supposedly preventing it, transmitted paralytic rabies to humans.

Of these eleven cases, two were reported in England. In a letter he published in the *Daily Telegraph* on December 8, 1886, **Dr. Clarke** wrote:

*The deaths of these two young people (**Goffi** in London and Wilde in Rotherham), which occurred three weeks after a complete treatment at the teacher training college, constitute facts that must be examined with the strictest attention. It has been claimed that Wilde succumbed to pulmonary congestion; but this self-serving version cannot be accepted. The symptoms present the greatest analogy with those observed in **Goffi.** The intensive prostration, the general paralysis of all the organs, the sudden invasion of the disease, and the rapidity of death—all the symptoms present an almost absolute identity with those that **Mr. Pasteur** described and observed in the animals he inoculated and designated under the name of rabies paralysis.*

It seems obvious to me that these two individuals succumbed as a result of nineteen virus inoculations they received in Paris.

*I am convinced that young Wilde did not succumb to rabies, which was not transmitted to him by a dog, but that he died of rabies paralysis, which had been inoculated into him by one of **Mr. Pasteur's** assistants at the laboratory of the École Normale.*

Signed: Doctor **Clarke.**

Following these events, Professor **Peter** addressed the *Academy of Medicine*, stating in the sessions of January 4th and 18th, 1887, that, as **Pasteur's** anti-rabies medication appeared to him to be becoming perilous in its intensive form, he considered it his duty to speak out. He described, with all the necessary details, several cases of death from paralytic or laboratory rabies that occurred after complete treatment using **Pasteur's** intensive method, and he concluded that, in the various cases he had just reported, the subjects died from experimental or paralytic rabies in rabbits, and not from the furious and convulsive rabies of dogs.

He justified his conclusion with the following reasoning: "*Dog rabies, which is convulsive, transmits convulsive rabies to the bitten man, and we rightly conclude that it was indeed his bite that gave him it.*"

"Rabbit rabies is paralytic; when inoculated, it must be paralyzing. Therefore, when a man inoculated with paralytic rabbit rabies dies some time later with paralytic symptoms, one must logically conclude that it was the rabbit virus that caused the paralytic rabies. One must conclude this because paralytic rabies is extremely rare in humans and it is only since anti-rabies inoculations that it has become frequent, a fact denied by the pasteurian school."

All these facts therefore establish that not only does **Pasteur's** anti-rabies treatment have no protective effect against rabies inoculated by dogs, but that it is likely to transmit paralytic rabies to rabbits.

In summary, not only does this treatment not cure rabies, but it can also transmit it to those who do not have it.

These facts are established without taking into account the numerous cases of death that have occurred abroad among subjects who came to Paris to be treated using **Pasteur's** method.

* * *

An eminently dangerous method was thus established. In its first application, it proved ineffective. It was then that **Pasteur,** without the slightest new experimentation on animals, without being guided by the slightest fact or the slightest well-founded biological basis, increased the daily number of marrows injected (3 per day for the first three days) and repeated 1, 2, or 3 times the series of the 3 or 4 most virulent marrows at a rate of one marrow per day.

But if **Pasteur** omitted to inform himself through new experiments on animals, another did; Professor **Von Frisch**, from Vienna, sent to Paris in April 1886 to study the method, returned with a favorable opinion. But he wanted to conduct control experiments before putting it into practice.

Those he carried out are beyond criticism; their results were in absolute contradiction with those predicted, announced, and trumpeted throughout the world by **Pasteur**.

Their formal result was that, when rabies inoculation in animals is carried out by the sure method of trepanation, Pasteur's preventive inoculations are powerless to prevent the appearance of rabies in dogs and rabbits.

In a communication to the *Vienna Academy of Sciences*, Doctor **von Frisch** declared that rabies can be transmitted by the intensive process, or at least by the rapid process, advocated by Pasteur.

This was therefore the formal annihilation of the principle on which the method of preventive inoculations was based.

Pasteur had therefore not carried out such experiments before putting his inoculations into practice because it is impossible to imagine that he could have resolved to do so if he had noted their absolute and constant ineffectiveness, an observation that he would inevitably have made if he had carried out **Von Frisch's** experiments, which were those dictated by the experimental method and common sense. **Pasteur** did not dispute the result of **Von Frisch's** experiments; he could not. But he disputed their significance and alleged, in a communication to the *Academy of Medicin*e, that if Doctor VON FRISCH failed in experiments of this kind, his lack of success was due to the slow vaccination method that he adopted.

And **Pasteur** indicates the conditions necessary for the success of such experiments. Here they are:

Vaccination must begin shortly after inoculation, the very next day, and it must be carried out quickly, giving the series of preservative marrows within twenty-four hours or even within a shorter period, then repeating the treatment once or twice every two hours.

Let us say first that, if **von Frisch** carried out his experiments with the slow vaccination method, it was to comply with the instructions and the procedure that **Pasteur** made known by inaccurately announcing that it infallibly ensured protection against rabies.

We have now arrived at a complete inconsistency:

Pasteur calls vaccination the inoculation of an animal with an exalted virus much more dangerous than the one it has already received less than 24 hours before. He calls preservative marrows the marrows 1 and 2 containing the exalted virus that surely transmit rapidly fatal paralytic rabies to a new dog or rabbit to which they are inoculated.

Finally, to cure a rabbit inoculated with the street rabies virus, it is also inoculated the next day with the exalted virus, which does not cure when it is inoculated gradually over 12 days.

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This new, extra-rapid process indicated by **Pasteur** can only give a result exactly opposite to the one he indicated. Indeed:

We know that when repeated injections of a virus, attenuated or not, vaccinate against itself, it is because it produces in the blood an agglutinating body called antitoxin, and this requires a minimum of 10 to 12 days to form.

Therefore, injections given over 24 hours at a rate of one every 2 hours cannot produce an antitoxin, the formation of which requires 12 days.

During the first eight or ten days of a disease caused by a heterogeneous virus or of repeated injections of such a virus, the virus multiplies and infects the patient; the appearance of the antitoxin is indicated by the cessation of the acute phase.

This is a general law, and it cannot be any different for repeated daily injections of rubic acid (red marrow). The antitoxin cannot appear until 10 or 12 days after the first injection, and during these 10 or 12 days, if the rabies virus from the biting dog has entered the body, it has developed there, and the antitoxin appears too late to act. This is what von **Frisch's** experiments showed.

It was therefore a mistake on **Pasteur's** part to have believed that repeated injections every two hours could accelerate the vaccinal state. This only appears in the body when the virulent agent has reached its maximum development.

The fact that rabies can develop several months after anti-rabies treatment proves, moreover, either that the antitoxin has no effect on the biting dog virus or that the anti-rabies treatment does not develop it.

Moreover, street rabies in dogs itself conclusively proves, through the death that occurs 20 days to several months after a bite or inoculation, that even if the street virus develops an antitoxin, it remains ineffective. It will be explained later why this antitoxin is ineffective and has no effect on the development of rabies.

It therefore seems quite obvious that with **Pasteur's** latest ultra-rapid process, only an accumulation of virus results.

It is therefore not surprising that the intensive method applied to humans in 9 to 12 days quickly had a catastrophic result, which manifested itself in the completely new appearance of paralytic rabies in the vaccinated.

The observation of eleven cases of paralytic rabies conferred on those vaccinated by **Pasteur's** anti-rabies treatment in 1886, on people bitten by rabid or only presumed rabid dogs, can be found in the work *M. PASTEUR et la rabie*, by Doctor **Lutaud,** editor-in-chief of the *Journal de Médecine de Paris* (Jules Lévy, 1887). This work is a very in-depth critical exposition of the pasteurian method.

It follows from the preceding facts that, from the end of 1886 and one year after the first vaccination carried out on the young **Meister** (July 6, 1885), the ineffectiveness of the pasteurian method was demonstrated by the death in France during the past year of 18 vaccinated subjects who nevertheless contracted rabies, the number of unvaccinated subjects who died of rabies being 17.

There were also 34 deaths among those vaccinated abroad.

On the other hand, the harmfulness of the method was established by 11 very clear cases of paralytic rabies, so-called experimental rabbit rabies, which occurred following vaccinations and which were obviously inoculated by them and not by the rabies virus of the biting dog.

How should we interpret Pasteur's statistics, which appear so favorable?

They must be interpreted in light of the following experimental facts:

1. When an animal is inoculated intracerebrally with the street rabies virus or the exalted virus by successive passages in the rabbit, it always contracts furious and convulsive rabies in the first case, paralytic or experimental rabies in the second;

2. When one or the other of these two forms of the virus is inoculated subcutaneously, rabies is transmitted to only one animal in 5 to 7. **Von Frisch** having inoculated the exalted rabbit virus under the skin of the necks of 6 rabbits, none of them contracted rabies;

3. Most bites are made by non-rabid dogs;

4. Rabid dog bites are less likely to cause rabies the shallower and more superficial they are, those of the hands and face being the most dangerous;

5. Rabies can only be confirmed in a dog if it can be successfully conferred on other animals by intracerebral inoculation of a portion of its medulla oblongata.

Diagnosis is more reliably established by keeping the animal alive and observing it than by autopsy, which can only provide a presumption;

6. Pasteur's statistics on the number of individuals bitten by rabid dogs contain unacceptable figures that are out of proportion with those indicated by official statistics prior to pasteurian treatment.

Professor **Colin**, of the *Alfort Veterinary School*, made a communication to the Academy of Medicine on November 9, 1886, to dispute that, as **Pasteur's** statistics indicated, 1,700 French people had been bitten by rabid dogs in a year and a few months; they were bitten by dogs, that is all that can be affirmed, because the information provided on biting dogs is collected by incompetent people and is neither checked nor even checkable. The number of cases where proof of rabies in the biting dog, or even an attempt at proof, has been made, represents only a tiny minority. On the other hand, even an autopsy performed by a veterinarian only gives a presumption and not a certainty.

Professor **Colin** thus proved that one must first deduct, from the considerable number of people bitten, an equally considerable number including people bitten by a non-rabid dog and for whom anti-rabies treatment proves absolutely nothing; that one must then deduct those who, although actually bitten by a rabid dog, do not contract rabies and whose number is very large, then those in whom the virus has been destroyed by cauterization.

From these considerations it must be deduced that **PASTEUR was totally ignorant of the number of subjects to whom the treatment was actually addressed**.

Alongside **Pasteur's** statistics, Professor **Colin** put those of the *Ministry of Agriculture*, compiled month by month and which indicate for the year 1885 351 people bitten, or an average of 29 per month.

This figure must approximately represent the actual number of those bitten, among whom only a small number will contract rabies. Proof of this is provided by the new statistics of cases of death from rabies, statistics which show that at that time the annual average of cases of death from rabies in France was 29 to 30.

The effectiveness of **Pasteur's** method should therefore be manifested by a notable decrease in the number of deaths from rabies during the year 1886.

The result was as follows: Instead of 29 to 30 deaths on average during previous years, there were 35, including 17 among the unvaccinated and 18 among those vaccinated by Pasteur; there were also 34 deaths abroad among those vaccinated.

In addition, among these deaths, there were 11 proven cases of paralytic rabies evidently conferred on subjects by inoculated rabbit marrow, cases in which death was not caused by the virus from the biting dog.

Faced with such results, any denial is completely useless, and only two conclusions are necessary:

1. The pasteurian vaccination method is ineffective and incapable of preventing the development of rabies once its virus has penetrated the body;

2. This method is dangerous, especially with its intensive application, and can communicate paralytic rabies to bitten individuals into whom the dog rabies virus has not penetrated.

* * *

Here, in summary, is the story of the development of this treatment method by reasoning alone and its implementation, without having previously established its foundations, bases, harmlessness, or dangers, by the experimental method.

Formal proof of this is provided by the fact that there is not a single publication by **Pasteur** that corresponds to such a determination. If one exists, please make it known.

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What does exist are experiments for obtaining the fixed virus and a process safe inoculation by the intracerebral route; however, the experiments of exaltation of the virus alone condemn the vaccination method.

The first experiments to control the method were those of **Von Frisch**, whose conclusions were diametrically opposed to those of **Pasteur.**

Despite this total absence of a reliable basis, of proof of the method's effectiveness, of proof of its safety, **Pasteur's** new sensational discovery was made known everywhere, throughout the world, through intensive propaganda in the press and by the most varied means; that of a safe, infallible, and completely safe vaccination process for curing or protecting against rabies.

And this safe, completely safe process involves the injection of the terrible exalted fixed virus, a single drop of which, injected into the brain, infallibly also causes paralytic rabies and death in an animal.

Intense publicity spread throughout the world in July 1985 when the first application of the method to young **Meister** was declared to have been saved from the terrible rabies virus, although in reality this application had no significance since it was completely unknown whether the virus had penetrated the subject or not, as not all bitten patients became rabid.

The method was then applied in the slow form, which presented the least danger, partly because of its slowness, and partly because it was presumed that a virus from the spinal cord series, attenuated by desiccation, conferred a slight resistance to the virus from the following, more virulent spinal cord or cords.

Under these conditions, no accidents occurred at first. Enthusiasm was everywhere, despite the advice for prudence and moderation given even at the Academy of Medicine when the accidents occurred, first with the little **Pelletier** girl, then with **Mermann**, who died of rabies, and finally with the Russians bitten by wolves, 3 out of 9 of whom died shortly after the inoculations.

These nine Russians, bitten on March 25 in Vladimir and cauterized, were treated by **Pasteur** starting on April 8. One of them died of rabies on the eleventh day of treatment, which was stopped on the fifteenth day and resumed later. He did not die because the doctor accompanying the inoculated patients decided to leave. A second Russian died of rabies during the trip, then a third a few days after returning to Vladimir.

These three deaths out of nine vaccinated people greatly impressed **Pasteur** and had a disastrous effect on public opinion.

Subsequently, the deaths multiplied, and through successive modifications of the method, **Pasteur** managed to make it so dangerous that it transmitted paralytic rabies to a certain number of vaccinated people, a fact that provoked the decisive intervention of Professor **Peter** at the Academy of Medicine.

This is the statement of the facts. This happened in 1886. Sixty years have passed, and despite what I have just explained, the fixed virus continues to be injected into unfortunate bitten victims and also into those who have never contracted rabies.

This is so because **Pasteur** wanted, despite all the failures, to affirm the excellence of his procedure; because he sought to prove, for each unfortunate case, that his procedure was not responsible; because false statistics were produced in which unfavorable cases were expurgated; and finally, because, for the insignificant cases, those bitten by non-rabid dogs, they were declared to have been saved from certain death by the treatment.

However, since 1886, numerous cases of paralytic accidents following anti-rabies treatment in humans, or even occurring during treatment, have been published in scientific journals and medical journals. These accidents are those of paralytic rabies in rabbits, which injections of dried bone marrow, known as pre-immunizing, transmit to humans.

But, despite this, we continue to assert the effectiveness of the treatment and the extreme rarity of accidents, because we distort their cause and because we always find excuses for not including them in the statistics.

The same is true of diphtheria toxoid, whose effectiveness and safety have been affirmed since 1923, although accidents are numerous and its effectiveness is nil, since vaccinated people contract diphtheria in proportions at least as great as unvaccinated people, and since instead of decreasing, it is constantly increasing.

Preventive vaccination of normal dogs against rabies has shown us what to think of the supposed safety of anti-rabies treatment.

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Of 8 dogs vaccinated with a formalin-based vaccine by **Plantureux** (of Algiers), 4 contracted paralytic rabies. Remlinger confirmed the fact and indicated that paralytic accidents are not rare in dogs undergoing anti-rabies treatment.

Here, there is no need to discuss since we are not talking about bitten dogs in which the vaccine would be ineffective, but about dogs that have only received the preventive vaccine that gave them paralytic rabbit rabies. And this same vaccine would not be dangerous for humans.

Nature of the rabies virus
Consequences resulting from this nature

Rabies is due to the growth, in the substance of the nerve centers, of a hyphomycete whose conidia are the **Negri** corpuscles that are observed in the brain of a dog that has died of street rabies. It has been doubted that these **Negri** corpuscles have a direct link with the rabies virus because the filtrate of an emulsion of brain substance retains its virulent properties even though these corpuscles are too large to be able to pass through the filters.

The explanation for this fact is as follows: The conidia of rabies *Aspergillus* are packed with fine round micropores which, like those of the conidia of the hyphomycete agent of smallpox and vaccinia, pass through the filters and are the direct agents of virulence, being capable of reproducing the hyphomycete and its conidia.

I affirm that this hyphomycete is the street rabies virus because it developed spontaneously on the brain of a dog that had died of street rabies, which **Mr. Vasseur**, head of the veterinary service at the pound, had made available to me. The *Aspergillus* had developed precisely on the surface of a tangential cut made at the level of the Ammon's horn, which is considered to be the region richest in **Negri** corpuscles; this surface emerged above the level of the formalin solution in which the brain was immersed.

An emulsion of the conidia of this *Aspergillus*, injected under the dura mater of a dog, caused the clinical signs of rabies in him and, on the third day, an extremely violent fit of furious rage during which he died after biting the wooden bars of the floor of his cage.

There could therefore be no doubt about the specificity of this *Aspergillus*. The mycelial nature of the virus now allows us to affirm that:

The antitoxin that can be developed by the street virus or the exalted fixed rabbit virus, attenuated or not by desiccation of the marrow, can have no effect on the development of rabies because antitoxins only act on bacterial cultures, whose elements they agglutinate to cause them to pass into the mycelial form, the development of which they accelerate.

Now, rabies is caused by the development, in the nerve centers, of the *Aspergillus* mold, whose spores are **Negri's** corpuscles. The antitoxin that the rabies virus could develop, attenuated or not, can therefore only accelerate this development. Under these conditions, anti-rabies vaccination is impossible.

The only thing that remains unknown in this question is the original source of *Aspergillus*. This source is not the one I indicated in the first volume of this work, which is erroneous. The determination is currently under study.

The mycelial nature of the virus explains the length of the incubation period, 20 to 30 days, the duration of the passage of a virus to the chronic mycelial phase being at least 12 to 15 days; the duration of this incubation period therefore proves that rabies is the manifestation of the disorders of the chronic and mycelial phase of the rabies virus, disorders that the action of an antitoxin could only accelerate.

The shortening to eight days of the incubation period of twenty to twenty-five days of the street virus by successive passages in the rabbit's body is probably due to the progressive accelerating action of the antitoxin on the mycelial evolution of the virus.

The fact that inoculating the virus into different parts of an animal's body does not does not develop rabies does not mean that the virus did not act; it only means that the virus remained localized at the point of inoculation; the development of rabies two to six months after inoculation proves that the virus took this time to spread from this point to the nerve centers in the mycelial form.

This mycelial nature of the virus, which makes it impossible to vaccinate against it, demonstrates the profound error committed by **Pasteur** in the reasoning that led him to institute his vaccination method.

The virus being now sufficiently known from the indications I have just given and easy to isolate in the *Aspergillus* form by culturing thin slices or pulp of the Ammonian horn of a dog that died of street rabies, we now have the necessary elements to verify and establish with certainty that **Pasteur's** method of anti-rabies vaccination does not vaccinate and that vaccination against rabies is impossible.

As for the contamination by the fixed rabbit virus of vaccinated people bitten by a non-rabid dog, this is a fact made evident, firstly by the type of paralytic rabies they present and secondly by the impossibility of anti-rabies vaccination.

Under such conditions, the measure is necessary to prohibit anti-rabies vaccination to prevent it from claiming new victims.

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CHAPTER XVI

STUDY OF THE VARIOLA VIRUS, THE JENNERIA VIRUS, AND THE CHICKENPOX VIRUS

The first two viruses were already the subject of a study in the first volume of this work in 1926, from which I falsely concluded that their original source was organic potato mold. This determination resulted from the comparison of conidial forms of hyphomycetes obtained by culturing scabs of cowpox and smallpox pustules, with forms obtained by culturing potato parenchyma.

Subsequently, I had to carry out a very strict check of all the initial determinations I had made of the original source of disease viruses.

For smallpox and vaccinia, I continued my study in vain for two years, sacrificing a considerable number of animals, rabbits and guinea pigs, without being able to obtain from the potato a virus that vaccinates against the Jennerian virus. I therefore had to recognize that my first determination was wrong.

But my study had nevertheless been fruitful because it had led me to discover the smallpox and vaccinia viruses and shown that these two viruses, far from being filtering viruses, are two hyphomycetes whose mycelium and large conidia, 10 to 15 microns in size, abound in the pustules of smallpox and vaccinia and are so visible that one must wonder how anyone could have avoided seeing them, even at low magnification. The reader who examines the figures in plates 71 and 72, particularly figures 3, plate 71, 1, and 6, plate 72, will obviously ask this question.

I saw the vaccinia virus because I took a fully mature vaccinia pustule from a rabbit's lip, fixed it in 10% formalin solution, and made thin sections after paraffin embedding; these sections, stained with ferric hematoxylin, showed me the vaccinia virus beyond any doubt, even from this first microscopic examination.

The preparations photographed in plates 71 and 72 were the first to come before my eyes. It seems that no one had previously been curious enough to look for the virus in a section of the vaccinia pustule.

In any case, this first observation of the morphological state of the vaccinia and smallpox viruses dispels the notion of filtering viruses, established precisely by the fact that, it was believed, the active elements of the smallpox and vaccinia viruses have dimensions below the limit of visibility at the highest magnifications. However, the conidia of these viruses are up to 12 and 15 microns long and 6 to 8 microns wide.

It was mainly because we had not been able to see the viruses of smallpox, vaccinia, measles, and scarlet fever that we accepted the notion of filtering viruses. However, in the first volume of this work, I had already shown, as early as 1926, that the measles and scarlet fever viruses are two hyphomycetes whose elements I detected in the blood, elements that are up to 12 and 15 microns in diameter.

Regarding chickenpox, plate 73 of this volume shows the conidia and conidiophores that exist in considerable masses in the crusts of the pustules, which they constitute almost entirely.

For hyphomycete cultures, the filtering elements are the fine, round microspores with which the conidia are stuffed.

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The notion of a filtering virus is therefore false, or rather, as I already explained in the first volume, all viruses are filtering. Almost all bacterial species include granular, cocciform elements, 0.2 to 0.8 microns in size, which are an integral part of them and which pass through the porcelain filters. Since the culture is filtered, the filtrate containing these cocciform elements is what has been inaccurately called a toxin, in application of another false notion dating from **Charrin** and **Bouchard**, according to which the filtrate of a culture is toxic due to the soluble poisons secreted by the bacteria.

It is not soluble poisons that constitute the activity of the filtrate; it is the cocciform elements it contains. If the filtrate is deprived of these elements, it loses all activity, and it can be progressively deprived of them by adding a precipitate of gelatinous tricalcium phosphate followed by centrifugation. The cocci and microspores are mechanically carried away by the granulations of the precipitate where they can be found. By repeating the operation as many times as necessary, the filtrate or toxin completely loses its activity.

I have also demonstrated the accuracy of these conclusions by establishing that diphtheria and tetanus toxins reconstitute in 24 hours all the elements of the original complete culture by the evolution of the cocciform elements into germinal masses and then bacillary forms. This demonstration is made in the first volume of this work dating from 1926.

But, for the pasteurian school, new discoveries are a dead letter; it does not sell progress in biology and bacteriology because it knows that they will lead to the collapse of the shaky scaffolding of false pasteurian dogmas and the death of serums and vaccines.

Variola Virus

The mycelium and conidia can be easily seen by removing the crust from a mature smallpox pustule, softening it by immersing it in water for several hours, then dissociating it and staining it with dahlia or methylene blue.

We can see that the conidia, slightly oval, are 6 to 10 microns wide, on average, and 10 to 15 microns long (fig. 1, 2, 3, pl. 70, gross. 550). Some are still equipped with their conidiophore. We see two of them in this case in figure 2, plate 70. We will see that these two conidiophores appear exactly similar to those of a hyphomycete of the Spicaria form that I collected from a smallpox crust cultivated by simple deposition on agar and of which figures 6 and 7 at magnifications of 245 and 610 are photographs. Another similar hyphomycete, of the same conidial type *Spicaria* photographed in figure 8 (gross. 245) was collected from the crusts of another case of smallpox cultivated on agar.

The conidia are packed with microspores about 9.8 to 1 micron in size, which often give their surface a bumpy appearance. One can be seen in the lower right corner of box J. 2 of the identification grid.

I also obtained other conidial forms by culturing smallpox scabs. They will be photographed in the first volume, along with various bacterial forms obtained either directly from the scabs or by culturing these conidial forms. I will not return to them here because, with the variola virus and its form known, there is only one thing that matters: to know its original source, to isolate it, and to verify that, by inoculating it, it vaccinates against the effects of the Jennerian vaccine.

Vaccine or Jennerian Virus

Figure 4, Plate 70, shows the elements of a scab from a dissociated human vaccine pustule, at a magnification of 550. The conidia, which are oval, are smaller than those of the variola virus. Like the latter, they are packed with microspores of 0.8 to 1 micron, just as yeast vesicles or cells are packed with globulins.

One of these granules can be seen in the center of square F3, two others in the upper left corner of square K7 in Figure 4, Plate 70. In the upper corner of squares C16, C16, C17, and H17, conidia full of microspores (magnification 550).

These microspores are the active elements of the vaccine. They are capable of completely reproducing the hyphomycete virus. They pass through the porcelain filters.

Figure 5, Plate 70, shows in BC 2 a bouquet of conidiophores identical to the bouquet HI 6 of Figure 8. Furthermore, in square E 5 of Figure 4, we see a conidiophore whose form is found in those of Figure 8, Plate 70, facts which tend to show that the vaccine hyphomycete is probably also of the conidial form *Spicaria*. This latter form of conidiophores is shown to be exactly similar in sections of rabbit vaccine pustules. We see one of these conidiophores in square I 13 and another in square M 11, of Figure 2, Plate 72, both bearing their conidium. Figures 4 and 5 of Plate 71, as well as Figure 4 of Plate 72, show that the mycelium and conidia deeply invade the dermis.

Figures 1 to 4 show the complete destruction of the dermis, in the vaccine pustule, by the mycelium, which remains alone in place with its conidia when the dermis has disappeared. Figure 6 of Plate 72 shows this mycelium.

Research continues to determine the original source of this hyphomycete, which, once well known and isolated, will be able to constitute the rigorously pure smallpox vaccine if, in the future, the continuation of this vaccination still seems desirable, which is not certain, the practice of vaccination should only result from a comparison between the importance of the risks of contracting the disease by the natural route of ingestion of the virus and that of the risks by the vaccination route, with which one **certainly** contracts the chronic phase of the disease. Since the remote complications of this are currently unknown to us, we are not in a position to make such a comparison.

On the other hand, prophylaxis against the variola virus will be considerably facilitated by knowledge of the original source of the virus, a source which is, with all certainty, a human plant food.

Chickenpox Virus

Plate 73 shows photographs of a preparation of dissociated chickenpox pustule scabs. Such a scab is made up almost exclusively of the conidia, conidiophores, and mycelium of the hyphomycete that is the virus.

The conidia have an average size of 4 to 8 microns, are oval, and are filled with microspores, like those of vaccinia and smallpox. A few are still adherent to their conidiophore. This is often arched, dilated at the base, and tapers toward the tip; it is also often straight and quite short. Looking through the photographs with a strong magnifying glass, the reader will see a multitude of these conidiophores, some still adherent to a fragment of mycelium that gave rise to it.

Since the potato is not the original source of the vaccine and variola viruses, it follows that the tomato is certainly not the original source of the chickenpox virus as I determined in the first volume; this source is certainly a food very similar to that whose ingestion causes smallpox.

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CHAPTER XVII

DETERMINISM OF THE PHENOMENA OF ANAPHYLAXIS. THE THEORY OF IMMUNITY. NATURE OF ALEXIN.

The injection of serum from one animal into another of a different species develops in the latter a state of anaphylactic sensitization and serum sickness.

In the first volume, in 1925, I had already established a nearly complete determinism of the phenomenon of sensitization and the mechanism of anaphylactic shock. One of the elements of the phenomenon had already been explained by **Abderhalden:** the development of a proteolytic ferment in the blood of the sensitized animal. But the false dogma of the asepsis of living beings and a false conception of the nature of ferments, also resulting from this dogma, formally opposed progress on the subject. Currently, the mechanism of anaphylaxis is completely clarified:

1. By the discovery of the bacterial nature of the fibrinferment or serozyme;

2. By the identification of the fibrinferment with Colibacillus, from which results the knowledge of the colibacillary function.

These new notions establish:

1. That sensitization is developed in the recipient animal by the injection of the fibrinferment or organic colibacillus contained in the antigen serum;

2. That the fibrinferment or organic colibacillus of the antigen animal develops and multiplies in the blood of the recipient animal as long as the state of sensitization persists, that is, for decades;

3. That the properties of the fibrinferment or colibacillus antigen injected into the recipient act on the fibrinogen and globulins in the recipient's blood to form an agglutinating fibrin, which is the antitoxin of the inoculated colibacillus. This antitoxin is therefore anti-colibacillary for the injected animal;

4. Why does a second injection of the antigen serum cause anaphylactic shock? It is by immediate agglutination of the colibacillus elements of the antigen serum, and by that

of the other colibacillus elements of the colibacillus antigen developed before the second injection into the recipient's blood.

5. Why does the blood pressure of the receiving animal drop immediately and sharply, as does the body temperature? This is because the agglutinated masses of the antigen colibacillus clog the capillaries of the organs and nerve centers, considerably reducing blood flow and causing general anoxyemia, that is, asphyxiation of the animal, cessation of exchanges, and hypothermia, even though the arterial blood has remained bright and rich in oxygen.

The mechanism of anaphylactic shock, thus completely understood, is therefore very clear and very simple.

So, in summary:

1. It is the organic colibacillus of animals (fibrinferment, serozyme) which is the sole and exclusive cause of the phenomena of anaphylaxis resulting from one or more successive injections of their serum;

2. The same is true for the injection of all organic matter from animals and which contains their organic colibacillus;

3. The albuminoid substances of the blood have no active effect in anaphylactic phenomena. The injection of serum from the blood of an animal would be totally powerless to sensitize an animal if it could be **completely** deprived of all colibacillus granulation. The albuminoid substances of the blood play only the passive role of serving the development of the injected colibacillus antigen and the elaboration of the anti-colibacillus agglutinating matter called antitoxin.

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THE DOCTRINE OF IMMUNITY

Pasteurian doctrine of the creation of immunity is a false dogma. Immunity against a disease does not exist. A person who has had smallpox and whose pustules have healed, or those who have had scarlet fever or diphtheria and are apparently cured, are not actually cured, and they remain subject to the attack and damage of the virus, which can manifest itself as long as the so-called immunity lasts, which is only the second phase of the disease, its chronic phase, still very little known to doctors for most diseases caused by a heterogeneous virus.

We know that when this so-called immunity is to occur, it is acquired after two weeks, three at most, like the state of anaphylactic sensitization after an injection of serum from another animal species.

Now, the well-known accidents that occurred precisely several weeks after vaccinations with diphtheria toxoid, B.C.G, or after injections of serum that gave the patient equine colibacillosis, are manifestations that can no longer leave any doubt about the persistence of the virus.

Currently, medical literature has made known to us so many unfortunate cases followed by death that it is impossible to dispute that heterogeneous viruses remain alive in humans after the end of the acute phase of the diseases they cause. They are widespread throughout the body, and the damage they cause can manifest itself at any point. The November 6, 1926, article in the *Journal des Praticiens,* reported on page 280 of this book, cited striking examples of this damage, twenty years ago.

Let us examine the damage caused by the invasion of the mycelium of the smallpox and vaccinia virus in the dermis, which it completely destroys at the level of the pustules (see plate 71), and we will realize whether it is possible for this virus to remain alive in the human organism without causing damage during the decades that immunity lasts, that is, the living state of the virus.

The characteristic type of virus with a long chronic phase is the syphilis virus. I isolated this virus, which I made known twenty years ago; it is not **Schaudinn's** *Treponema pallidum*, which is only a spiral appendage, a tendril of its mycelium; it is the real virus that the pasteurian school has refused to recognize and use for twenty years, thus clearly demonstrating its indifference to scientific progress and its close collaboration with the campaign of suppression undertaken by **Roux** against my publications since 1926.

The discovery of the true syphilis virus, described on page 622 of the first volume of this work and photographed in plates 283 and 284, is an acquisition of capital importance for the fight against syphilis. It reveals that its virus is a heterogeneous hyphomycete and provides the weapon that allows us to search for its original source. It also provides the means to search for the virus itself, which is cultivated and reproduces with extreme ease in ordinary broth, and the means to destroy it. Moreover, possession of the virus allows us to perfect the **Wassermann** reaction to a high degree by providing the ideal antigen to carry it out.

This is what the pasteurian school refused to use and sought to suppress. The exceptional seriousness of this attitude is underlined by the fact that on the occasion of another discovery of such capital importance, that of the nature, origin and autogenous development of Koch's bacillus, a tissue-building dumbbell organelle, the pasteurian school delegated its staff and students to come and insult me during a conference where I was developing this discovery at the Museum before the scientific world.

Let's not dwell on this and return to our subject. The syphilis virus is indeed heterogeneous, always coming from outside. It vegetates in the chancre in its pure mycelial form; it is its mycelium that constitutes it. The disease is therefore mycelial and chronic from the outset; the mycelium spreads throughout the organism and causes, throughout the individual's life, varied damage which can reach, as we know, any point or organ; none escapes it. This is therefore the type of chronic disease of long duration caused by a heterogeneous virus. Plate 71 of this volume demonstrates the destructive activity of the vaccinal and variola viruses.

It is therefore certain that these viruses produce varied damage in the organism, for example nephritis, disorders of the liver, of the various glands, of the nervous system. Is not the whole series of numerous diseases of the brain and spinal cord, encephalitis, various myelitis, due to disorders caused by the mycelium of heterogeneous viruses such as those of smallpox and vaccinia, those of diphtheria viruses, those of eruptive diseases, that of poliomyelitis which is related to diphtheria viruses, etc.?

Are not arteriosclerosis, cerebral softening due to the mycelium of these viruses?

The paralysis and nephritis following diphtheria and poliomyelitis, scarlet fever and other eruptive fevers, are in any case clear proof of the existence of the chronic phase in these diseases.

The accidents that follow vaccinations prove that they communicate this chronic phase; this is, moreover, obvious and incontestable since the goal of vaccination is to install the live virus in the organism and since it is known and classic that it no longer vaccinates if its vitality is too attenuated.

The terms immunity and vaccination are therefore empty words that have a false meaning. Immunity by vaccination is only acquired by conferring on the individual the chronic phase of the disease that one wants to avoid, a chronic phase which involves, in the recent or very distant future, formidable complications.

The new notions contained in this book show that medicine is still very backward and above all that, since the development of the harmful pasteurian doctrines, its progress has not only been stopped, but that it is above all the accumulation of a large number of new errors that has constituted the evolution of medicine for three-quarters of a century.

I am convinced that well-directed studies will lead to knowledge of the original cause that determines, for example, certain progressive muscular paralysis of young people, encephalitis, myelitis, various nephritis of all ages, **Bright's** disease, vascular diseases, etc.

I am also convinced that the damage of the chronic phase of diseases caused by heterogeneous viruses considerably shortens human longevity.

What is the reason for the so-called immunity? A whole series of antibodies has been used to explain it. In reality, the entire mechanism of what has been taken for immunity is the formation in the blood, by the heterogeneous virus itself, of an agglutinating albuminous material, a fibrin, which has the effect of agglomerating the elements of the virus into germinative masses and promoting its transition to the mycelial state, coinciding with the cessation of bacterial multiplication.

This substance persists in the body as long as the virus is alive. If a second infection occurs, it agglutinates the bacterial elements that have entered the body and alleviates the disorders of the acute or bacterial phase or, at the very least, greatly reduces its duration and intensity.

But this agglutinating action, which always occurs at the end of the acute period of a heterogeneous virus disease, **does not have the effect of destroying or annihilating the virus, but only of causing it to transition to the mycelial state, the form in which it will vegetate in the body during the chronic phase**.

In a word, this agglutinating body that produces immunity and causes the bacterial virus to transition to the mycelial form is the antitoxin whose presence has been observed in so-called antidiphtheria and antitetanus protective serums.

The accuracy of this mechanism is verified in vitro by direct action of the antitoxin on the antigen.

* * *

Autogenous diseases do not vaccinate. These are those caused by the deviation of one of the body's two organelles; cancer and tuberculosis are caused by the deviation of the tissue-building dumbbell; colibacillary diseases, erysipelas, phlegmon, abscesses, osteomyelitis, phlebitis, leukemia, tetanus, gangrene, etc., are caused by a deviation of the micrococcal form of the organic colibacillus.

These diseases do not vaccinate because the tissue-building dumbbell organelle and the micrococcal granulation of the colibacillus are normal elements of the body and their functioning is regulated in such a way that it is never hindered. A normal element of the body cannot vaccinate against itself, because this vaccination would cause rapid death.

When one animal's fibrinferment (organic colibacilli) is injected with its serum into another, the latter is vaccinated against it. If it is injected a second time with the antigen serum, it is killed in three to five minutes by anaphylactic shock. This is the same shock that would be produced by the creation of the autogenous vaccinal state against a normal organelle.

Vaccination against an autogenous disease is therefore impossible, and it can be posited as a principle that the fact that an animal's disease does not vaccinate is proof that it is autogenous, with the exception of heterogeneous diseases of a mycelial nature from the outset, such as syphilis.

The development of an autogenous bacterial disease in an animal was believed to be impossible because **Pasteur,** through inaccurate experiments and erroneous statements, had established the false dogma of the asepsis of living organisms. It was to keep this false dogma inviolable that the pasteurian school fought any scientific research establishing the normal existence of bacteria in the body, particularly symbiotic bacteria of **Portier**.

But now that the eminently bacterial nature of living beings is firmly established and rendered incontestable by the identification of fibrinferment with colibacillus, then by the identification of the latter with staphylococcus, streptococcus, pneumococcus, enterococcus, septic vibrio, tetanus bacillus, etc., the autogenous development of an abnormal bacterial culture in the organism is fully explained and its original cause becomes luminously clear.

CONCLUSIONS

1. The word immunity does not mean that an immunized animal can no longer contract a given disease, but only that it is in the potential of the chronic phase of this disease and that, if it is infected a second time, the acute phase will be nil or benign;

2. The word vaccination does not mean that by vaccinating, the vaccinated person is permanently protected against the dangers of a disease; it only means that the live virus is introduced into the body of the vaccinated person and that the chronic phase of the disease that the vegetation of the vaccine will develop in him is conferred upon him. The multiple cases followed by death that have followed inoculations with diphtheria toxoid provide proof of the existence of this chronic phase and its dangers;

3. The mechanism of this so-called immunity that renders the acute phase of a second infection benign is the formation by the antigen, in the blood of the immunized person, of an agglutinating fibrinous substance (antitoxin) that acts on the bacterial elements that cause a second infection and brings them together into clusters that will thus form germinative masses that will cause the virus to pass directly into the mycelial state. This very simple mechanism is the sole cause of this so-called immunity, which is in reality only the existence of the chronic phase of a disease with all the complications and damage, sometimes formidable, that it can cause.

CONSTITUTION OF ALEXIN

The composition of alexin was studied at length in the first volume of this work, but was not completely elucidated. The identification of fibrinferment with colibacilli now makes it possible to clarify the mechanism of complement action, an action that was attributed to a body called alexin without ever knowing its composition.

When an animal's serum is dialyzed against spring water, the elimination of the NaCl it contains causes the precipitation of globulins. When the liquid is centrifuged, the precipitate settles at the bottom of the tube. The serum or complement is thus split into two parts, which are separately inactive but become active again if the precipitated globulins are redissolved in the resalted liquid part.

German authors have designated the globulins precipitated by dialysis as **Mittelstück** or middle part of the complement, and the liquid part deprived of its globulins as **Endstück** or terminal part of the complement.

The globulin precipitate or **Mittelstük** is thermolabile and loses its activity if kept for 15 to 30 minutes at a temperature of 57-580, or longer at a lower temperature. Stored in the tube where it was centrifuged, and in its compact form, the globulin precipitate retains its activity for a fairly long time; if it is redissolved in 7.5% salt water, this activity disappears fairly quickly; this is the aging of the **Mittelstück** or **Brandt** phenomenon, completely identical to the aging of thrombin because the latter is formed by the same body as alexin.

The liquid part of dialyzed serum or **Endstück** is thermostable and does not break down even at 1000.

Here are the explanations that explain what alexin is and the mechanism of its action:

The thermolabile body that possesses the activity in the **Mittelstück** is the micrococcal granulation of the fibrinferment, or colibacillary granulation, which also constitutes the serozyme. It ages quite quickly when suspended in the complement because it evolves, vegetates, and transforms, as in serum left at room temperature, into chains of Streptococci.

As for the **Ensdtück** or terminal part of the complement, which is thermostable, it is the lecithin of the blood.

Complement or alexin is therefore neither fibrinferment or serozyme, nor lecithin, but only the result of the hydrolysis of the latter by serozyme, that is, the entire acid portion of lecithin, released during the hydrolysis reaction, and consisting of glycerophosphoric

acid and palmitic and oleic acids. These acids, acting on them in their nascent state, possess a powerful hemolytic action on blood cells.

This hemolytic action increases in power if the blood cells are first covered with a thin layer of a special fibrin, the hemolytic sensitizer, which is eager for the acid group released from lecithin and binds it energetically.

In summary, alexin and thrombin are the same, and both represent not a defined body, but only a chemical reaction: the dissociation of lecithin by fibrinferment, colibacillary granulation. In both cases, whether it is the coagulation of fibrinogen, or the natural or specific hemolysis of blood cells, the directly active body is the acid group of lecithin, glycerophoric acid and oleic and palmitic acids, or others, depending on the constitution of the lecithin.

It is known that globulins separated and then added to the complement decrease or even eliminate its activity if a sufficient quantity is added. This action is explained by the fact that they are eager for the acid group of lecithin, which they monopolize, thus preventing it from playing its hemolytic role. Their action is therefore exactly identical to that of adding a certain quantity of alkali to the complement, which also prevents hemolysis.

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CHAPTER XVIII

CONSEQUENCES FOR EPIDEMIOLOGY OF KNOWING THE ORIGINAL SOURCE OF VIRUSES

CAUSES OF THE STALLED PROGRESS FOR THREE-QUARTERS OF A CENTURY

For almost seventy-five years, the fight against human diseases and bacteriology have made only insignificant progress, and epidemiology has made none.

One only has to read the studies and reports concerning epidemics in scientific periodicals and in the *Bulletin of the International Office of Hygiene of the League of Nations* to realize that doctors and bacteriologists have never discovered the slightest clue that sheds light on the cause of the appearance or disappearance of an epidemic, and even less on the cause of its progressive spread.

These reports have indeed been the subject of a considerable number of decrees, circulars, and regulations enacting prophylactic measures, such as the closure of schools, mandatory reporting, isolation of the sick, disinfection of premises, etc., ineffective, useless measures, the result of which has been to complicate and greatly hinder the existence of those who are not sick and which have so far provided doctors and civil autho-

rities with no other resource than to watch helplessly as the epidemic develops, dying out as it began, without them knowing why, and without the measures they take having the slightest influence on its evolution.

Prophylactic measures are almost useless for the following reasons:

1. Because we isolate subjects suffering from diseases such as tuberculosis, sweating sickness, poliomyelitis, etc., for which it has been recognized that contagiousness is zero and not noticeable, even for subjects living in the same room, sometimes the same bed;

2. Because the pasteurian dogma of contagion is false, and because it is not through contact with hands, clothing, or the projection of saliva that epidemic diseases such as smallpox, chickenpox, scarlet fever, measles, poliomyelitis, diphtheria, etc. are usually contracted; it is almost exclusively through the ingestion of food, the original source of the virus, that infection occurs.

There are indeed a few very rare cases of contagion from one sick individual to another; but they are a tiny minority; during the 1927 polio epidemic in Romania, **Marinesco** was unable to identify more than 1 or 2 cases out of 446 where direct contagion could be incriminated, and even then it is not certain that they were not related to the ingestion of the food-virus. However, polio does have a frankly epidemic appearance;

3. Because we persist in pouring torrents of formaldehyde and other antiseptics onto floors and walls, in sterilizing linen, clothes, and mattresses in an oven, an operation prescribed by an absurd, backward bacteriological science, rendered incapable by false pasteurian dogmas, while the virus will continue to claim other victims because it lives in the kitchen, for diphtheria and polio, for example, in a small box of cereal flour or in cow's milk, which simply need to be stopped to achieve radical prophylaxis, without any other measures, without serum, without vaccines, nor isolation, nor disinfection, nor school closures.

The cause of the development of typhoid fever epidemics, the cause of isolated and distant contaminations are far from being clear and well established; bacteriology tells us that **Eberth's** bacillus exists in drinking water during epidemics. But it is not the water that produces it. The answer to this objection is that it is brought into the water by the excrement of the sick.

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It can be objected again that in an isolated village, or even in a town, the first patient could not have contracted the disease from the excrement of another who did not exist. In this case, where did **Eberth's** bacillus come from? Bacteriologists, who have fertile imaginations and are accustomed to answering with a series of ready-made clichés, will then invoke the carriers of latent germs, the latent germs deposited here and there on

organic matter...etc. These answers do not satisfy the question, because they refer, identically, to the germ carrier.

As for the existence of the typhus virus that would roam on organic matter, this is one of the heresies of bacteriology, one of the results of its backward state caused by the false pasteurian dogmas; unable to explain it with facts, it explains it with hypotheses.

There are no autonomous bacterial viruses with a very ancient existence and fixed specific characteristics.

All bacterial or mycelial elements that exist free and wandering in nature come from a current living organized being that rejected them during its lifetime or liberated them by its death. Once liberated, they undergo multiple morphological and biological modifications, this through the play of the extreme fundamental polymorphism of living matter. On the other hand, these liberated living germs do not live long; they die quickly, either because they do not find materials favorable to the maintenance of life, or by the action of light, etc.

The origin and source of all bacterial and mycelial elements is the organism of living beings, animals and plants, because their two constituent organelles are bacterial in nature and one of their functions, of capital importance, without which their life is impossible, is the bacterial function.

Like all disease viruses, the **Eberth** bacillus is the agent of the bacterial function of a living being, animal or plant. Is it autogenous, that is, the organic colibacillus of man, deviated in its properties, or is it the agent of the bacterial function of a heterogeneous organism? This is what remains to be determined with certainty.

As long as the living being, which is the original source of the **Eberth** bacillus, has not been determined with certainty, there will be no certain prophylaxis of typhoid fever.

* * *

The absolutely essential condition for progress in epidemiology is therefore knowledge of the original source of viruses, these being the active agent of the bacterial function of an animal or plant, which one only needs to know to avoid contamination by ingesting it.

The search for such knowledge was therefore made totally impossible and even prohibited by the four pasteurian dogmas of atmospheric panspermia, the asepsis of living organisms, bacterial monomorphism, and contagion. It was therefore equally impossible for epidemiology and bacteriology to make any progress, this being completely dependent on four principles which are exactly:

1. The existence of living virulent germs in living beings and not in external environments and the atmosphere;

2. The bacterial nature of living beings;

3. Bacterial polymorphism and polymorphism of living matter in general;

4. The absence of contagion between individuals, contamination taking place exclusively by ingestion of food viruses or inoculation through the skin (syphilis) in the case of heterogeneous diseases or, in the case of autogenous diseases, by degeneration of the two constituent organelles, the colibacillus (colibacillary diseases) or the cell-building dumbbell organelle (tuberculosis and cancer). The only exception to this rule is contamination by biting insects.

From these principles it follows that, in most cases, epidemics do not result from the transmission of the disease by contact between individuals, but only from the simultaneous ingestion of the same virulent food by a group of individuals in a region of a city or over a certain area of territory.

This is a factor that has only been taken into account for diseases whose propagation by water is accepted, cholera and typhoid fever for example.

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For diphtheria and the group of eruptive fevers, there are certainly never epidemics by contagion, contamination occurring through ingestion of the food which is the original source of the virus, which does not mean, of course, that there cannot be, **exceptionally**, cases of disease by contact and direct transmission from a sick person to a healthy person. For these diseases, consider, for example, the case of a batch of special cereal flours for children's food, delivered to a single district of a city, a batch coming from a region where cereal crops have been badly affected by cryptogamic diseases, anthrax, *Cladosporium*, rusts, etc.

These flours will develop diphtheria in this district alone; schools will be closed as a prophylactic, but this will be totally ineffective, because it is the ingestion of the flours, the cause of the infection, that would have to be prohibited to eliminate all new cases.

It is truly astonishing that no one has noticed until now the coincidence that exists for certain diseases, diphtheria, poliomyelitis, measles, etc., between the average age of the sick subjects, the type of diet (consisting mainly of milk and cereal flours), and the entry point of the virus, the pharynx, whose lesions are constant at the onset of the disease.

Since the viruses of heterogeneous diseases are made up of food, and especially plant foods, it is on these that research must focus in particular in epidemics where the virus and its original source are unknown.

We will see the importance of the food factor in two cases that we will examine as examples: poliomyelitis and the plague.

Infantile Poliomyelitis

The study of investigations into polio epidemics reveals that the dietary factor, of such vital importance, has gone completely unnoticed. However, clinicians have observed that, at the onset of the disease, there are very significant lesions of the tonsils, salivary glands, and nasopharynx, which represent the primary lesion and are the portal of entry for the virus.

This portal of entry indicates that it is with food that the virus penetrates. **Ionesco-Mihaesti's** statistics (35) on the polio epidemic that raged in Romania in 1927 contain a very suggestive and important indication concerning the number of polio cases according to the age of the patients. Here is the table:

Out of 446 cases of polio, there were:
From 0 to 1 year: 65 cases.
From 1 to 2 years: 158 cases.
From 2 to 3 years: 116 cases
From 3 to 4 years: 62 cases.
From 4 to 5 years: 17 cases.
From 6 to 10 years: 8 cases.
Over 10 years and adults: 20 cases.

We have just explained that the virus introduced with food enters the body at the level of the tonsils and nasopharynx. We also see, from the table above:

1. That the disease affects almost exclusively children from 1 to 5 years, and especially those from 1 to 4 years;

2. That it is especially among children from 1 to 2 years that the cases are most numerous (158 cases), then, with a little less frequency, those from 2 to 3 years (116 cases), then those from 3 to 4 years (62 cases); it then falls to only 17 cases for children from 4 to 5 years;

3. That if, for children from 0 to 1 year old, there are only 65 cases, the lower number of this figure results from the fact that a large number of children fed with their mother's milk are only subject to the influence of the aliment-virus from the age of 6 or 8 months to 1 year; it is therefore at a proportion at least double that of 65 cases that we must estimate the number of cases of poliomyelitis in children from 6 or 8 months to 1 year old fed with cereal flour porridge and cow's milk, children from 0 to 6 or 8 months fed exclusively with their mother's milk remaining unaffected;

4. That this table accurately represents the frequency of feeding with cereal flour porridges from the 6th month; almost exclusively composed of milk and these porridges from the 6th or 8th month until the end of the second year, the feeding includes the increasing addition of foods of other kinds from the third year to the fifth.

It therefore becomes very probable and even certain that the poliomyelitis virus is a conidial form of the living matter of one or more of the food grasses used to make the porridges; the decreasing proportion of the number of cases of disease from the third year onwards agrees exactly with the progressive decrease in cereal porridges until the fifth year.

The infection occurs because the living matter in flour, as well as the spores of cryptogamic diseases it contains, and also the cow's milk used to feed children, are not subjected to boiling and sufficient temperature to ensure their sterilization.

It is regrettable that the preceding table does not contain several essential pieces of information: the number of polio cases from 0 to 6 months, then from 6 months to 1 year, as well as the number of patients who were fed exclusively on their mother's milk and those who were fed on cow's milk.

Indeed, cow's milk can contain the virus just as well as flour because animal bedding, made of cereal straw, contains the spores of cryptogamic diseases as well as the normal living matter of these plants, and it soils the udders, belly, and thighs of the animal when it lies on the bedding; we know that, under these conditions, milk is very often soiled with multiple fragments of manure. Cow's milk can therefore contain the virus just as well as flour.

On the other hand, if the virus is indeed, as I indicate, a conidial form of one of the food grains, there should be no poliomyelitis in children fed exclusively on their mother's milk until the time when cereal porridge and cow's milk are added to the milk diet (generally from the 6th to the 8th month).

Indeed, children cannot contract diseases caused by heterogeneous viruses as long as they are fed exclusively on the milk of the mother, who is completely free of them.

If, during an epidemic, there are infected children among those fed exclusively on cow's milk, this fact will constitute proof that this milk carries the virus.

It is therefore only from their diet of cereal porridge and cow's milk that children can contract poliomyelitis, that is, most often from the sixth month, for those who are fed on breast milk.

In the statistical table reproduced above, the lower number of cases (65) among children aged 0 to 1 year, compared to the 158 cases observed among children aged 1 to 2 years, must most likely stem from the fact that these 65 cases concern only children aged 6 or 8 months to 1 year, who are already receiving cereal flour porridge and who, consequently, must be the only ones to be contaminated. Taking this observation into account, the frequency of cases among children aged 6 or 8 months to 1 year must be as high as among children aged 1 to 2 years.

All statistics relating to poliomyelitis are silent on the exact nature of children's diet, something which is nevertheless of the utmost importance.

The importance of information on diet is not limited to poliomyelitis, but extends to all diseases caused by a heterogeneous virus and to any epidemiological study.

In the event of a polio epidemic, two methods can be used to stop its development: prolonged boiling of cereal porridge and milk or their replacement with vegetable purées.

From a culinary point of view, cereal porridges do not lend themselves to prolonged boiling, which makes them pasty, sticky, and too thick. On the contrary, a purée of mixed vegetables—lentils, peas, beans, potatoes, carrots, pumpkin, etc.—withstands a two-hour boiling without any inconvenience and ensures perfect nutrition for children; it also provides the surest way to avoid the disease.

Since this method is simple, easy to implement, without any inconvenience for the children, and no more expensive than any other, it is therefore this one that should be used and which must radically suppress the development of a polio epidemic.

But, to avoid the first cases of this disease, it is necessary that children's food be, at all times, subjected to sufficient boiling to destroy the vitality of the living matter and spores of cryptogamic diseases in cereal flour, milk, or any other food.

As for the exact original source of the virus, it can be known using the general method indicated previously.

It is good to supplement this indication with the following:

The healthy living matter of cereal grains, cultivated in vitro, always gives rise to several different conidial forms which, probably and even surely, produce dissimilar diseases upon infection. Now, poliomyelitis is a characterized disease, exhibiting a fairly great uniformity and therefore apparently caused by a uniform virus, although it may have its original source in the three aforementioned food grains, viruses of the hyphomycete group that cause diphtheria.

This indicates that it is not the normal living matter of the grain that, in general, must give rise to the virulent conidial form and that it rather comes from one of the abnormal transformations of this living matter, that is, from the spores of one of the cryptogamic diseases of cereals, smut, bunt, rusts, etc.

This conclusion is made probable:

1. By the fact that there would be epidemics every year, if it were the normal living matter of cereals that develops the virus, which is not the case;

2. By the fact that epidemics are rare and therefore correspond to exceptional causes that have contaminated cereal harvests. These causes are prolonged rains, associated with great heat which develop all cryptogamic diseases, storms which cause lodging which makes the grain lying on the ground moldy, prolonged rains preventing the bringing in of sheaves already harvested and causing the alteration of the grains;

3. The fact that there have always been sporadic, few cases of infantile poliomyelitis in all countries, which demonstrates that it is a virus that develops every year, in an accidental, irregular manner, and with variable frequency. This is indeed the case with cryptogamic diseases of cereals, anthrax, *Cladosporium*, bunt, and rust, which always contaminate cereals, but with great rarity in very dry years and great frequency in very wet years.

This is the long-observed influence of meteorological conditionson the development of epidemics.

It must be concluded from this that, in cases of epidemics, microscopic research of spores in cereal flours and in vitro culture of these to obtain conidial forms may perhaps provide valuable information.

Cereals were severely affected by cryptogamic diseases during the months of June and July 1946, a very rainy year in the northern half of France. We must therefore expect a resurgence of diphtheria, measles, scarlet fever, poliomyelitis, etc., during the coming winter and the year 1947, in France and Western Europe.

The plague from an epidemiological point of view

Let us now examine the case of the plague. The study of this disease in the endemic focus of the Kyrgyz populations of southeastern Russia was carried out in a very interesting way by **Nikanoroff** (53).

This scholar indicates (53, p. 648) that the Kyrgyz steppes comprise two regions whose soil is very different; one, whose soil is formed of earth consistent enough for animals to make burrows in, is inhabited by a large number of ground squirrels and mice.

In the other region, the soil is made up of shifting sands that are not consistent enough to make burrows in; this region, where, for this reason, ground squirrels are very few in number, is inhabited mainly by mice that lodge and make their nests in large clumps of a chenopodiacee cultivated in both regions, *Agriophyllum arenarium*, which the inhabitants call koumartchick.

In both regions, a fairly large number of ground squirrels and mice contract the plague, as do all other rodent species.

According to **Nikanoroff**, only ground squirrels contract the plague in the first region, where mice remain unaffected, despite frequent promiscuity, even in the same burrow, with ground squirrels. This fact seems abnormal and requires further monitoring since, in the sand region, up to 80% of mice have been observed to be contaminated.

In the country, the inhabitants have long noticed that the plague, which exists among them in an endemic state, spreads greatly during years when the koumartchick harvest is abundant. **Nikanoroff** writes: "*Usually, the plague is expected in the region where the koumartchick harvest has been good.* >

Some plague epidemics coincide with the harvest and threshing of koumartschik. In this case, the question arises of human contamination by spores from the cryptogamic formations of the stem, leaves, and grain of the plant, spread into the air by the harvest and especially by threshing, which heavily loads the air breathed by the individuals participating in the operation with spores and various dusts; the nasopharyngeal and pulmonary mucous membranes are always very heavily loaded with this dust after threshing.

Research carried out at the Saratov Institute of Microbiology revealed in 1924 that, while no ground squirrels were still infected in April, the number of plague-infected animals gradually increased until August, that is, the time of the koumartchick harvest. This growth is shown in the following table:

In April, out of 133 ground squirrels examined, 0, or 0%, were plague-infected.

- May, 1,842 examined; infected 11 or 0.6%

- June, 2,640 examined; infected 156 or 5.8%

- July, 968 examined; infected 88 or 9%

This growth seems to indicate that the development of plague in ground squirrels increases like that of a plant, the koumartchick for example, or like the cryptogamic diseases that affect it.

The cause of this increase in cases of contamination is related to that of plague epidemics that coincide with the threshing and harvesting of koumartchick, or that follow an abundant harvest of this goosefoot.

Human contamination by mice or ground squirrels is believed because plague epizootics that affect these animals are usually followed by an epidemic in humans or among camels. A connection has also been observed several times between contagion from mice to humans and haymaking, especially because field mice come to live in the har vested hay.

This conclusion of human contamination by mice, rats, or any other rodent goes beyond the significance of the facts observed. The fact that rodent contamination precedes that of humans does not necessarily mean that humans are contaminated by them, nor that camels are contaminated in the same way. The fact necessarily means, and nothing more, that rodents are the first to come into contact with the original source of the virus and, probably, that they come to feed on the grain or plant that contaminates them, even before it has reached maturity.

What matters most is not whether the rat contaminates humans, but what is the source of the virus that contaminates the rat, because, let us repeat once again, knowledge of a bacterial form of the plague virus is of little use: it is knowledge of the true form of the virus, of its original source, in a word, of the living being that forms the virus, that will immediately provide the radical means of eliminating the disease.

Humans and camels can ingest the virus through food just as well as rodents, and contract the plague from the same source as them, without being contaminated by them.

In the first volume of this work, I made an initial search for the original source of the virus, based on the comparison of its bacterial forms and its conidial form, *Penicillium*, with those of the rat. I concluded that the virus must be the living matter of the rat.

Although I am not yet certain, I think that my determination is not exact and that, perhaps, the culture used in my study was not a plague culture, but a culture of the living matter of the rat, due to the similarity of the bacterial forms reported in the first volume, but this is only a hypothesis.

In any case, my first study required a control that has not yet been able to be carried out. Currently, we are guided in research by a series of important new indications which are:

1. The fact that every pathogenic virus comes from a living being;

2. The fact that heterogeneous viruses are, almost all of them, the plant foods of man and animals and that, consequently, it is through food that the penetration of viruses into the animal organism takes place;

3. The fact that rodent infection by the plague generally precedes that of humans, and that all species of rodents are contaminated;

4. The fact that the main and most sought-after food by rodents is the seed of cereals or other plants, a fact which makes their infection by a conidial form of these likely;

5. The increase in cases of ground squirrel contamination during the months of April and July, with a peak at the maturity of these plants;

6. The coincidence between the harvest and threshing of koumartchick and the development of the plague;

7. The usual coincidence between the abundance of koumartchick and the development of the plague, a fact which also signifies the general abundance of grasses cultivated in the country and which may also be incriminated.

All of these facts call for the following experiment to be carried out, which will indicate whether the plague virus is indeed introduced into the organism of rodents by the grain of a cultivated plant, koumartchick, which is a chenopodiacee, or by a grass.

This experiment consists of capturing several batches of 50 non-plague-infected ground squirrels or mice in February or March, the time when they are free of plague; feeding one batch with koumartchick seeds, the litter being made from the same dry plant; feeding each of the other batches with the seed of only one of the other species cultivated in the region, the litter being the straw of this species, in order to introduce into the experiment the action of the spores of cryptogamic plant diseases; finally, keeping, as controls, a batch fed with other foods than the previous ones. The batch where the plague cases occur will provide an indication of the original source of the virus.

It is necessary, of course, to completely isolate the batches from each other to avoid contamination by fleas. Furthermore, the animals must be subjected, before the experiment, to a diet other than that of the plants designated above and, during this time, subjected to a control to eliminate plague-infected animals.

The infection of ground squirrels and mice with plague is due to one of their most common favorite foods. Knowledge of this food will provide a secure basis for plague prophylaxis in the land of the Kyrgyz; by abandoning the cultivation of the plant, which is the original source of the virus, the endemic focus of plague will be definitively eliminated from this region.

* * *

In summary, progress in epidemiology is absolutely dependent on knowledge of the original source of pathogenic viruses.

We cannot defend ourselves against an enemy whose form, properties, or exact location are unknown. But we can defend ourselves against an enemy whose form, quality, and location are known.

Since the location of the viruses and the name of the material or food that constitutes them are known, it is sufficient, for heterogeneous diseases, not to ingest them in order to avoid contracting these diseases or, if they are ingested, to destroy their virulence by sufficiently high and prolonged heat. If we want, for example, diphtheria and poliomyelitis will very quickly disappear from the list of diseases using these means, without any serum or vaccine.

We can posit as a principle that, in any epidemic, the disease never develops by contagion between individuals, and that, if numerous sick people scattered far from each other contract the same disease at the same time or successively, it is exclusively by ingestion of the same food spread throughout the region and which is the original source of the virus.

It is established, for example for sweating sickness and poliomyelitis, that these diseases are not contracted by contact with sick people.

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When several members of a family are affected simultaneously or successively, there is no proof of contagion; there has only been simultaneous contamination by ingestion of the same food. It is certainly to this that we must attribute the fact reported by **Marinesco** that, during certain poliomyelitis epidemics in the American States, family receptivity reached the figure of 40%; in reality, there was no family receptivity or contagion, there was only simultaneous ingestion of the food-virus by members of the same family.

Conversely, from the fact that in a family of two or three young children, only one of them contracted the disease despite all having ingested the same viral food, it should not be deduced that this food is not the virus, the cause of the infection. This results from multiple causes, from the conformation of the nasal cavities, that of the pharynx, especially that of the tonsils, etc., and also from chance, that is, from the fortuitous adhesion of particles of a viral food in certain parts of the mouth or pharynx, or from a coughing fit that will have caused these particles to enter the nasal cavities. Children do not all have the same conformation of the nasopharynx, which explains why they do not all have the same degree of receptivity.

In summary, the suppression of heterogeneous infectious diseases and the epidemics they cause depends on the original source of their virus. Once this source is known, it will be easy to avoid them either by refraining from consuming them or by destroying their virulence through suitable heat action.

CHAPTER XIX

HARMFUL INFLUENCE ON THERAPY OF FALSE PASTORIAN DOGMAS

AND THE IGNORANCE OF THE COLIBACILLARY FUNCTION FOR WHICH THEY ARE RESPONSIBLE

Pastorian dogmas having falsely taught physicians that the human organism is aseptic and that any bacteria that penetrates it is heterogeneous, they have endeavored, following the example of the pastorian school itself, to discover:

1. Chemical substances that kill or stop the development of heterogeneous bacteria;

2. Serums or vaccines that exert a similar action against these bacteria or a protective action against them.

It turns out that one of the bacteria most frequently found in the body is E. coli. Since the pasteurian school teaches that intestinal colibacillus comes from outside and that, when it invades the body, it is because it has crossed the intestinal mucosa, multiple therapeutic attempts have been made to find chemical substances and anti-colibacilli vaccines, and these are now part of the arsenal of common medications and vaccines; the most toxic of these medications are those of the arsenobenzol type and sulfona mides.

Against tuberculosis, creosote, gold salts, etc. have been used.

Against syphilis, substances of the arsenobenzol type (Salvarsan) or bismuth salts have been used.

Among these substances, some, such as salvarsan, have caused serious and fatal accidents. A doctor from the Paris hospitals who, about ten years ago, had already had to treat 18 cases of poisoning by this drug, informed me that they were all fatal despite numerous successive blood transfusions.

These accidents were the consequence of a profound alteration of the coli-bacillary function and the haltere organelles of the blood cells by salvarsan.

The question of treating tuberculosis with antibacillary products is currently being considered by clinicians and has been more or less abandoned.

On the question of syphilis, I will not dwell on it, because, in this very special disease, it is difficult to act and, since action is necessarily necessary, the search for a treatment,

even with chemical products, is not questionable provided that their toxicity is well studied and their dose regulated.

A new study of antisyphilitic drugs is absolutely necessary from the point of view of the mechanism of their action, and especially from the point of view of their toxicity towards the organic colibacillus.

* * *

Those who sought a chemical treatment for colibacillus diseases, and those who applied it, with faith in the accuracy of pasteurian dogma of asepsis of the living organism, were unaware that this colibacillus, which they sought to kill or neutralize, is a normal element of capital importance in the human organism, one of the two organelles that constitute it and the active agent of the colibacillus function that carries out all the diastatic actions of the animal organism.

If the ingested drug is truly active, that is, harmful and toxic to the colibacillus, the certain result of the treatment will be a profound and dangerous intoxication for the patient, whose colibacillus function will be seriously impaired, destroying the colibacillus cocci, the leukocytes and platelets, and also the blood cells. The alteration of other anatomical elements, notably those of the nervous system, is also probable.

It is important to understand, now that the colibacillary function is known, that toxicity of a substance primarily means toxic action on the active elements (cocci) of the colibacillary function.

Fortunately, most of the drugs currently used are not very toxic; but this low toxicity leads to absolute ineffectiveness; here is an example: a member of my family, bedridden due to a fracture of a lower limb, was struck down with phlebitis. One of the drugs known to be one of the most active and specific as an anticolibacillary was immediately administered. After 15 days of treatment with a dose that should have been active, the patient developed urinary colibacillosis. This drug is therefore completely inactive.

From these facts, it follows that, due to the existence and the capital importance of the colibacillus function, if an anticolibacillus drug is truly active, it risks killing the patient to whom it is given and it can only be seriously harmful. As for graduating the action of such a drug, this would obviously be dangerous and very risky; one should therefore not give an active anticolibacillus drug, even if it is a little.

If the drug is not active against the colibacillus, there is no point in ingesting it; it is useless, unless it has the property of modifying the reaction of the blood, in which case it

can act usefully on the vegetative activity of the colibacillus in the excreted or exuded fluids.

The same is true for all products claimed to be active against staphylococcus, streptococcus, pneumococcus, and enterococcus, which are all the cocciform granulation of colibacillus and which can all transform into typical colibacillus.

Pasteur's false dogmas and ignorance of the crucial function of colibacillus have led researchers, and in particular laboratories specializing in the sale of vaccines and serums, to develop them against colibacillus, streptococcus, pneumococcus, etc., while ignoring the fact that these supposedly different bacterial species are actually one and the same, and, for example, that an anti-streptococcal vaccine that is active would also vaccinate against pneumococcus, staphylococcus, and colibacillus.

Moreover, the laboratories developed these vaccines while ignoring the fact that they were seeking to vaccinate against the normal bacterial forms of one of the two organelles that make up the human organism, the active agent of the crucial colibacillus function, an illusory attempt that is a misinterpretation.

What shows the confusion, absurdities, and misinterpretations that the false pasteurian dogmas have led to, and the rudimentary state of current bacteriology, is that the organic colibacillus, a crucial constituent of the animal organism, against which vaccines and serums claim to act, appears in several morphological forms, one of which is the leukocyte, to which bacteriologists attribute precisely high phagocytic and microbicidal properties against this same colibacillus and against its staphylococcal, streptococcal, and pneumococcal forms, of which it is one of the elements and one of the phases of evolution.

If they acted, the serums and vaccines in question would therefore have the effect of destroying the leukocytes, which, on the contrary, it is believed they are helping.

Thus, with regard only to the specific point of organic colibacillus, the harmful dogmas of atmospheric panspermia and the asepsis of living beings have prevented us from achieving knowledge of:

1. The bacterial nature of living beings;

2. The organic colibacillus, and by this fact, of a physiological function of capital importance;

3. The nature and functions of leukocytes;

4. The identity between colibacillus and its various micrococcal forms, staphylococcus, streptococcus, pneumococcus, etc.;

5. The identity between colibacillus, the septic vibrio, the tetanus bacillus, *Bacillus lactis aerogenes*, etc.;

6. On the autogenic nature of gas gangrene, tetanus, and all colibacillary diseases, pneumonia, erysipelas, etc.

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Instead of acquiring these crucial notions, these dogmas and other erroneous notions of bacteriology have led to the use of toxic substances dangerous to colibacillary function and to the preparation of vaccines and serums intended to act and immunize against this normal and essential constituent of the organism, the colibacillus.

One cannot imagine a deeper fall of a science into error, absurdity, and ridicule.

Bacteriology having retained the same false dogmas and principles without evolving for three-quarters of a century and having been knowingly maintained by the pasteurian school in the rudimentary state in which it currently finds itself, it was inevitable that it would lead to results as absurd and as unfortunate for the reputation of French science and for the progress of the fight against diseases; equally inevitable that its edifice, supported on false foundations, on errors of observation and reasoning, on false pasteurian dogmas, would one day collapse; at this moment, this edifice is collapsing under the weight of accumulated absurdities.

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CHAPTER XX

IS THE OBLIGATION OF VACCINATION AGAINST TYPHOID FEVER, DIPHTHERIA AND TETANUS JUSTIFIED?

DID THE VALUE OF THE VACCINES INOCULATED JUSTIFY SUCH A MEASURE?

Unfortunately, this question involves extra-scientific elements and considerable material interests. Although it is painful for me to address this question as I am about to do, it is necessary for me to show, in defense of the public's interests, what path was taken and what means were employed to finally succeed in decreeing, in violation of everyone's freedom to self-determination, the odious obligation for the French to have their children inoculated against their will with virulent, dangerous microbes devoid of any vaccinating power.

The goal of the pasteurian school's ongoing struggle against any new notion contrary to pasteurian false dogmas is the safeguarding of considerable material interests and the fear of adverse consequences for the manufacture and sale of vaccines.

The day after the lecture I gave in the Museum's main amphitheater on February 25, 1937, on the nature and autogenous origin of Koch's bacillus and tuberculosis, Doctor

Fougerat, secretary of the Social Hygiene League "Life and Light," wrote a letter to Doctor **Julien Noir (1)** in which the following passage appears:

"Tissot is touching on something other than a scientific question and considerable material interests that would be ruined if his concepts turned out to be real.

"You can be sure that the conspiracy of silence will redouble and that everything will be done to break the man."

This opinion of Doctor **Fougerat** proves that the motives, the ultimate goal of the struggle engaged in an attempt to stifle the results of my scientific research since 1926 have not escaped the medical profession, which has clearly understood that this is a matter of material interests rather than a scientific question.

What are these material interests?

Doctor **Charles Nicolle**, professor at the Collège de France and director of the *Pasteur Institute* in Tunis, informed us about this during an interview published in the February 3, 1934, issue of *Nouvelles littéraires, artistiques et scientifiques*, where he described the state of the *Pasteur Institute* at that time.

After stating that "ROUX did not manage the Pasteur Institute well, and concluding that the institution is gradually taking on the appearance of an establishment for oriental civil servants constantly on leave," he adds:

The Pasteur Institute is fortunate to have a financial base: the serum and vaccine preparation service. Modernized, it would generate greater profits, which would allow it to return to the State the subsidies it pays. It is better to have the State as a client than as a protector. We are still assured of regular revenue and avoid encroachments.

So these are the material interests at stake: the sale of serums and vaccines prepared by the Pasteur Institute.

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Already, in the first volume of this work, I indicated and proved that vaccination by introducing live viruses into the human body was a dangerous process and that the method of choice for achieving prophylaxis against diseases caused by heterogeneous viruses was knowledge of their original source, which makes it possible to avoid their ingestion or destroy their vitality before ingesting them and, thus, to surely avoid the disease without the need to introduce its live virus into the body.

It was mainly this notion, as well as that of the bacterial nature of living beings, from which it resulted, which prompted Professor **Roux**, director of the Pasteur Institute, to ban the publication of my memoirs at the Academy of Sciences and the *Biological Society* in 1926.

(1) *Letter published in a pamphlet by Doctor Fougerat entitled: Are There Dogmas in Science, and in which he protests against the stifling of freedom of opinion, discussion, and scientific publication*

* * *

It is deplorable and disastrous, for the interests of progress in medicine and science, for hygiene, and for the public, that a scientific institute recognized as being of public utility is at the same time a commercial house that manufactures serums and vaccines and lives off the profits made.

Like a merchant, such an institution is always inclined to avoid modifying and perfecting an object when it generates a very remunerative profit; it is also inclined to encourage consumption of the objects sold and to use all means to increase it. The campaign stubbornly pursued at the *Academy of Medicine* and with the public authorities to make vaccinations against diphtheria compulsory for children and in the army had no other goal.

Since the financial basis of the Pasteur Institute is the sale of serums and vaccines, it is absolutely essential, at all costs, that it sell a lot of them. This is why all the activity of this Institute is directed towards a single goal, which is the search for new serums or vaccines that are profitable, while at the same time increasing the sale of old vaccines.

This is the reason that motivated the campaign undertaken to bring about compulsory vaccination, which was to prodigiously increase the financial return of the three inoculated vaccines.

Since 1921, attempts had been made to generalize and even make compulsory the vaccination of infants with a vaccine that was not only ineffective, but capable of causing accidents: B.C.G. It is to **Lignières** that we owe having prevented this generalization, and we only narrowly escaped a catastrophic compulsory vaccination.

It was also inevitable that, in order to increase the return on "its financial base," this Institute would seek to acquire predominant influence in the various official hygiene commissions and at the Ministry of Public Health.

This exposes what I have called the extra-scientific elements and material interests that come into play in the question. Let us now move on to another.

* * *

Since serums and vaccines have been manufactured, have there been attempts to extend and perfect knowledge relating to the tetanus and diphtheria viruses?

The facts present themselves exactly as if all research on these viruses had been prohibited or avoided for fear that it would reveal facts detrimental to the sale of serums and vaccines.

The chapter of this book entitled: "The Struggle of the Pasteurian School Against the Truth for Three-Quarters of a Century" provides information on the tenacious desire of this school to maintain, despite everything, and against all evidence, pasteurian errors.

When knowledge of the nature of the diphtheria virus and one of its original sources: barley, was published in 1926, the director of the *Pasteur Institute*, **Roux**, instead of seeking to use this crucial new concept, sought only to stifle it by banning my publications, because he feared that this concept would harm the sale of anti-diphtheria serum.

Also, no progress has been made on this virus; we have remained with the diphtheria bacillus, whose nature, formation, origin, and where it lives are unknown. Incredibly, no attempt has even been made to determine the composition of the false diphtheria membrane, which, however, contains the secret of the form and knowledge of the original virus and its main sources, Cladosporium herbarum of cereals, barley, wheat, rye, of which the diphtheria bacillus is only a secondary formation of the mycelium.

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All this highlights the antagonism that exists between disinterested scientific research, which must perfect the means of combating diseases as much as possible, and the exercise of a business selling serums and vaccines. Such a business is inevitably conservative and the enemy of progress.

The indisputable proof of the disastrous effect that commercialization has had on this Institute is the backward, lamentable state into which it has allowed bacteriology to fall, which is currently collapsing under the weight of accumulated errors and absurdities, knowingly maintained, which are listed in Chapter V under the title: "*The Errors of Bacteriology.*"

Now, what must be taken into consideration in this matter are not the financial interests of the *Pasteur Institute*, but exclusively the interests of the public sacrificed by it and by the Ministry of Public Health when it made compulsory a vaccination that was not justified from any point of view, not even by real or sufficient effectiveness, as the studies in this book on the diphtheria and tetanus viruses prove.

My intervention is prompted by the fact that, after having, to protect the sale of its vaccines, strangled freedom of scientific opinion and publication, which should be even more respected than freedom of political opinion, the *Pasteur Institute* violated individual freedom of self-determination by imposing, for false and worthless reasons, the obligation to vaccinate children against diphtheria, then arbitrarily extending this obligation to other vaccines, all by usurping an authority it does not possess.

The law of June 25, 1938, on compulsory vaccination against diphtheria

The law making diphtheria vaccination compulsory is illegal because it violates the principal right recognized to the French by the Declaration of the Rights of Man of 1789 and by the Constitution of 1875, that of freely disposing of one's person.

As Dr. P. Chavanon **(1)** rightly explains:

A surgeon does not have the right, even in the case of an operation essential to saving a child's life, to perform it without the parents' written authorization, and one would claim to have the right to forcibly and without authorization inject a harmful product that could kill a healthy child!

Add to that: a totally ineffective and useless product developed without sufficient studies, without any control of its effectiveness.

By what series of maneuvers was it possible to obtain the vote for such a law? It is by a series of totally inaccurate and false assertions, which I will expose:

First, an attempt was made to impose the measure on the army through intense propaganda at the Academy of Medicine. Through individual influence, a majority was formed that voted for a resolution asking the government to decree vaccination in the army.

Thus was done, and once this first step was completed, active propaganda began to overcome the second: compulsory vaccination for children. A second time, two false and mendacious assertions succeeded in obtaining the vote for the odious law of June 25, 1938; this was by falsely asserting that vaccination is harmless, that it permanently protects children from diphtheria, and that it would make this disease disappear from our country.

The law was passed in 1936 in the Chamber and in 1938 in the Senate. To obtain the vote in the Senate, the rapporteur, **Mr. Léculier**, used mendacious arguments that must be made known and which are indicated by the following passage from the *Official Journal* of June 3, 1938:

Mr. François Saint-Maur: It is clearly understood that, as with ordinary vaccination, the operation will be performed free of charge, the costs being borne by the State.

Mr. Even, President of the Commission: Mr. Léculier's report provides the answer to Mr. François Saint-Maur's question.

(1) *Dr. P. CHAVANON: Your child can be killed. Éditions Médicis, p. 79,*

There is no need to provide for financing in the current text since the distribution of expenses would be carried out according to Article 26 of the law of February 15, 1902, between the State, the departments, and the municipalities.

From a financial point of view, the expenses will certainly be offset by the elimination of the 2,000 annual deaths and the treatment, each year, of nearly 20,000 cases of diphtheria.

A large part of these currently existing costs falls on public budgets (expenses for free medical assistance or social insurance). The costs of vaccination will certainly be less than the savings they will generate by the elimination of diphtheria.

These are the lies that were presented to the senators to influence their vote.

At the time of the vote, regarding the disappearance of diphtheria through vaccination, the number of cases of diphtheria had doubled since it was put into practice in 1923, and it had been proven that vaccination does not protect and that vaccinated people contract diphtheria just as easily as unvaccinated people. Furthermore, the harmfulness of toxoid and the falsehood of the claim that it was harmless had been proven.

Currently, the proof that these are indeed false claims is given in a peremptory manner by the new facts, presented in this book, establishing:

1. That diphtheria viruses are multiple and that diphtheria toxoid, responding to only one virus of unknown nature, is rigorously incapable of protecting against other viruses which are specifically different from each other;

2. That the initial diphtheria virus is not the **Klebs-Loefler** bacillus, but a hyphomycete of the forms *Cladosporium, Citromyces,* etc., originating from the transformation of the living matter of cereal grains or the conidia of their cryptogamic diseases;

3. That, consequently, the original source of diphtheria viruses is the flour of various cereals;

4. That the diphtheria false membrane is constituted entirely by the mycelium of the hyphomycete virus. The fact that no research has been done to elucidate the nature and constitution of false membranes demonstrates the notorious inadequacy of the research that led to the development of the serum and toxoid.

In summary, it is a notorious negligence, an almost total absence of research on the qualities and nature of viruses, and also on the efficacy and safety of serums and vaccines that presided over their development. The research that should necessarily have been carried out before the implementation of anti-rabies, anti-diphtheria, anti-tetanus vaccinations, and also of all other vaccines, is that contained in this book.

The practice of these useless, illusory, dangerous, and unjustified vaccinations has become a national danger that urgently needs to be eliminated.

Is the odious attack on freedom by this illegal law at least justified by the certain protective effect of the diphtheria and tetanus vaccines?

NO, THESE VACCINES DO NOT VACCINATE

We will examine this ineffectiveness successively for the diphtheria, tetanus, rabies, and B.C.G vaccines, then the question of the usefulness and appropriateness of typhoid vaccination.

DIPHTHERIA VACCINATION

The ineffectiveness of this vaccination is proven peremptorily:

1. By the fact that diphtheria viruses are multiple, at least six and even nine specifically different, and that the diphtheria toxoid, responding to only one of them, cannot vaccinate against the others;

2. By the fact, practically established and beyond all dispute, that vaccinated subjects contract diphtheria in a proportion at least equal to that of unvaccinated subjects.

To this ineffectiveness, the following disadvantages must be added:

1. Inoculating children with toxoid certainly gives them diphtheria, which they would have contracted only in the proportion of about 1 in 100 children. The chronic phase of this diphtheria, which lasts for many years, exposes them either to close accidents that can lead to death, or to distant and multiple accidents;

2. Inoculation quite often causes the fever of the acute phase of the disease.

The balance between the advantages and risks of vaccination gives the following result:

Advantages: None; toxoid does not vaccinate, does not confer protection.

Risks: Certainty of contracting diphtheria inoculated with toxoid and of being exposed to the serious risks of its chronic phase, which, although rare, can be fatal in the short term. Faced with this certainty, there is a risk of about 1% of contracting diphtheria, but a risk still incurred since toxoid does not vaccinate.

Conclusion: Since the risks that vaccination poses to vaccinated people are considerable and well established, and its total ineffectiveness is incontestable, it must not only be removed as a requirement, but also prohibited, since it serves no purpose other than to cause accidents, sometimes fatal.

Let us add that the use of horse anti-diphtheria serum is subject to the same criticisms that peremptorily establish its ineffectiveness. Moreover, its use should be prohibited because it inoculates the patient with equine colibacillosis, which lasts as long as the state of anaphylaxis that is this colibacillosis itself and which exposes for decades to the accidents of its chronic phase.

TETANUS VACCINATION

Its total ineffectiveness results from the following facts:

1. Tetanus is an autogenous disease and, consequently, does not vaccinate, that is, does not protect against subsequent attack;

2. The blood of a man cured of tetanus does not contain antitoxin (**Vincenzi**);

3. Tetanus toxin, a filtrate of an anaerobic tetanus culture, exposed to oxygen, reverts to its original bacterial type, the colibacillus;

4. The colibacillus, the agent of normal colibacillary function, is one of the two fundamental constituent organelles of the animal organism;

5. Since the colibacilli of various animal species are specifically different, the antitoxin developed in the horse's blood by inoculation with the tetanus bacillus from the autogenous tetanus of one animal cannot be active against the autogenous tetanus of another animal. It is active only against the strain of tetanus bacillus or toxin that was used to immunize the horse and only in the case where its mixture with it is injected into an animal of another species (experimental tetanus). This antitoxin is inactive against the autogenous tetanus of the animal that provided the strain of bacillus or its toxin because, in this case, it is entirely fixed by the considerable mass of the organic colibacillus of this animal;

6. From these facts it follows that the results of experiments on heterogeneous experimental tetanus are not applicable to humans.

These facts explain the total and inevitable ineffectiveness of tetanus toxoid, and they explain the equally total and known ineffectiveness of antitetanus serum.

In the leaflets accompanying the bottles of antitetanus serum, the *Pasteur Institute* advises immediately injecting a massive dose of serum in cases of declared tetanus.

However, it is well known at this Institute that **Vaillard** and **Roux** have demonstrated since 1893 the total ineffectiveness of antitetanus serum in this case.

In summary, tetanus toxoid does not and cannot vaccinate, since a tetanus attack does not itself vaccinate. Antitetanus serum is completely inactive because tetanus is autogenous.

On the other hand, both the antitoxin and the antitetanus serum safely inoculate the patient with colibacillosis: that of the horse's colibacillus in the case of the serum, that of the colibacillus of an unknown animal in the case of the toxoid, the *Pasteur Institute* unaware of the origin of the tetanus bacillus culture he has been using since the beginning of serum production.

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Under these conditions, since toxoid and serum are completely inactive and only serve to inoculate humans with a very long-lasting colibacillosis and expose them to the accidents of its chronic phase, their use must be prohibited.

Let us note here that tetanus develops just as well after preventive injections of serum; forty years ago, Professor **Berger** had already been able to collect 35 cases of tetanus developing despite preventive injections. But it was necessary, to ensure the sale of the serum, to claim that the doctor's responsibility was engaged if he did not perform the injection for the slightest injury.

This is a self-serving assertion whose effect must be destroyed. In reality, this injection is totally ineffective and powerless to prevent the development of tetanus.

Secondly, this being known, it is the injection of inactive serum, dangerous because it can provoke a fatal attack of anaphylaxis, dangerous because it certainly inoculates the horse colibacillosis, which engages the doctor's responsibility and which, apart from moral responsibility, can expose him to lawsuits for damages at the very least. Let him remember that tetanus is autogenous and does not vaccinate.

RABIES VACCINATION

This vaccination has been shown to be ineffective:

1. By its disastrous results recorded in the first year of its application in vaccinated individuals who contracted rabies and died;

2. By the fact that vaccine injections of rabbit marrow transmit to the vaccinated the paralytic rabies of the rabbit that the bite of a rabid dog does not transmit;

3. By the fact that attempts to vaccinate normal dogs have proven that, in a high proportion, it is not a vaccine action that develops, but rabies itself that leads to the death of the animal;

4. By the control experiments of **Von Frisch** (1887) which demonstrated that **Pasteur's** method is incapable of preventing the outbreak of rabies and that, on the contrary, it can give it to those who would escape its evolution;

5. By the fact that rabies is caused by the development, in the nerve centers, of a hyphomycete of the *Aspergillus* type that I isolated and whose inoculation under the dura mater causes furious and convulsive rabies and whose spores are Negri's corpuscles.

- This mycelial nature of the rabies virus implies the inability of an antitoxin to stop it, which, on the contrary, can only accelerate the development of the mycelium; it leads to this conclusion: that it is impossible to vaccinate against rabies due to its mycelial nature, and its character as an accident of the chronic and mycelial phase of the virus's evolution.

As a result of all these facts, anti-rabies vaccination must be prohibited to avoid new victims.

ANTITYPHIC VACCINATION

This vaccination is contraindicated by the comparison between the risks it poses and those of contracting the disease. For the Paris metropolitan area and its suburbs, for example, an average of 1 in 15 or 20,000 inhabitants contracts typhoid fever each year.

It is to avoid this extremely minimal risk that the vaccinated person is given, without fail, the chronic phase of the disease and even, in quite a few cases, its acute phase which, although attenuated, is not always negligible.

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One of the big mistakes of current medicine is to believe that infection is over a heterogeneous virus when the temperature has returned to normal and when the disorders of the acute period have disappeared. In reality, the disease has only passed from the short bacterial stage to the long mycelial stage of the chronic phase, which lasts for decades and whose symptoms can be much more serious than those of the acute phase. This is the same error one would make in believing syphilis to be cured when the initial chancre has disappeared.

The immediate effects of typhoid vaccination are too well known to require repeating them here. The disorders that persist after the apparent cure of typhoid fever, the state of deficiency of the organism that often persists for many years, the disorders or after-

effects caused by the growth of the mycelium of the infecting agent in the organs, are reasons that should prohibit typhoid vaccination, especially in the army and in the civilian population during epidemics.

It is primarily a commercial motive that drives claims about the benefits of vaccination. In reality, the serious risks it poses—the guaranteed contamination of the vaccinated person by a very dangerous virus that will live in the body for decades—are reasons that do not bear comparison with the negligible risk of 1 in 15 or 20,000 contracting the disease.

I recall, just for the record, that in the first volume of this work, I concluded that grasses are the original source of typhoid fever (corn), paratyphoid fever A and B (wheat and rye), and exanthematic typhus (oats). The ongoing verification of these determinations will certainly provide new details that will provide new and effective means of prophylaxis.

ANTI-TUBERCULOSIS VACCINATION BY BCG

Chapter XIII of this volume establishes that vaccination against tuberculosis, and in particular by B.C.G, is impossible for the following reasons:

1. Because tuberculosis is an autogenous disease that does not vaccinate, the virus being constituted by the haltere organelle, the builder of human tissues, deviated in its properties;

2. Because the bovine tubercle bacillus is the dumbbell organelle that builds the tissues of the ox, deviated in its properties, and is incapable of vaccinating against bovine tuberculosis (failure of **Behring's** experiments);

3. Because the human tubercle bacillus is, like the normal dumbbell from which it derives, specifically different from the bovine tubercle bacillus and the normal bovine dumbbell;

4. Because the B.C.G, having lost the tuberculizing property by culture in a bilious medium, has thus apparently returned to its original type of normal dumbbell and cannot vaccinate humans against a property that it no longer possesses;

5. Because tuberculosis is a chronic disease from the outset and a mycelial virus against which vaccination is impossible;

6. Because, under these conditions, the B.C.G cannot even vaccinate against the bovine tubercle bacillus.

Let us add to these facts that, since bovine colibacillus is virulent for humans, in whom it causes serum sickness because it differs specifically from human colibacillus, it is certain that B.C.G, the dumbbell organelle of cattle, is specifically different from the dumbbell organelle of humans and virulent for them.

In summary: Effective vaccination with B.C.G is impossible, as it is not a vaccine and is virulent for humans. Under such conditions, this vaccination must be prohibited.

* * *

The *Pasteur Institute* is currently (June 1946) distributing a leaflet to the medical world, accompanied by a letter inviting doctors to spread propaganda in favor of B.C.G vaccination. This letter and this leaflet contain false statements that are strictly contrary to reality and truth, such as this: "It is permissible to state in the most formal manner that vaccination is completely safe and effective. To claim the contrary, one must be either misinformed or acting in bad faith."

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The *Pasteur Institute* is currently making the same statement about diphtheria vaccination, although all doctors are now convinced that this toxoid has no vaccination power, that vaccinated people contract diphtheria at least as easily as unvaccinated people, and that vaccination causes disorders and accidents, some of which are fatal.

The *Pasteur Institute* claimed that diphtheria vaccination would eliminate diphtheria in France, and on the contrary, the number of cases has continued to increase.

It is therefore a bias at the *Pasteur Institute* to systematically deny the best demonstrated and proven facts. The same is true for B.C.G.

The virulence of B.C.G is established by the numerous accidents it has caused and which have led to its abandonment, but which **Calmette** systematically denied despite their obviousness.

As for a protective power, it cannot exist because of two fundamental conditions necessary for the creation of immunity, which B.C.G does not satisfy:

1. The bovine bacillus from which B.C.G originates is specifically different from the human tuberculosis bacillus;

2. The B.C.G, having definitively lost its tubercular power, has at the same time lost the essential quality for it to immunize against tuberculosis.

In addition, tuberculosis is autogenous, spontaneous, and due to the abnormal growth of a normal constituent element, the tissue-building dumbbell, against which one cannot vaccinate.

These are three errors made in the development of B.C.G, which, therefore, is not a vaccine; it has reverted to the normal ox dumbbell type, and that is all.

The author of the prospectus in question, director of the B.C.G department, faithful to his establishment's instructions to stifle and excommunicate, pretends to ignore my 1926 publication on tuberculosis, the nature and origin of Koch's bacillus, the autogenous nature of tuberculosis proving that it is not through contagion that one contracts tuberculosis, as clinicians of **Pidoux's** time had clearly seen.

But his silence will not prevent the new concepts contained in this book, combined with those of the second volume, from demonstrating throughout the world the absolute impossibility of the B.C.G's vaccinal power.

CONCLUSIONS

Let's now summarize the facts: The diphtheria, tetanus, rabies, and B.C.G tuberculosis vaccines have no vaccination power and do not protect. The typhoid vaccine, which is eminently dangerous, reliably (100%) inoculates the chronic phase of typhoid fever with its dangers, in order to avoid the insignificant, almost zero risk of contracting it, 1 in 20,000.

These vaccines are all dangerous and cause accidents that can be fatal.

These facts establish that these vaccinations have created a very serious situation and that they constitute a national danger that calls for rapid action: first, the abolition of the law of June 25, 1938, making diphtheria vaccination compulsory for children, and then of the law concerning tetanus vaccination.

But this measure is insufficient: The five vaccinations I have just listed must be banned, as must the use of anti-diphtheria and anti-tetanus serums, which inoculate the horse colibacilli and are completely inactive. This ban is the only effective solution to the problem.

The public can also quickly resolve this by refusing to vaccinate children. It has the right to do so because the law only requires diphtheria and tetanus vaccinations; it is through abuse of power by the *Pasteur Institute* that another vaccination has been added: typhoid vaccination, which is not mandatory for children.

School principals therefore do not have the right to refuse admission to school to a child who has not undergone the triple vaccination.

But it is above all doctors who have the duty to protect the public against these vaccinations, first by refusing to perform them, and then by having their unions, associations,

or medical councils request their abolition, and then by advising the public to put the following principle into practice:

One must not, under any pretext, allow oneself to be inoculated with a live virus, even an attenuated one, nor with a serum, nor with any product derived from living beings, with the exception of chemical products free of figurative elements.

This is the principle adopted by the Netherlands and England by refusing to make them compulsory.

Now that the ineffectiveness and dangers of vaccines are known, will the French continue to accept that their children are treated as guinea pigs for the sole reason of the financial prosperity of an establishment?

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